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DOMESTIC VENTILATION.

A LESSON FROM THE COAL-PITS.

By W. Mattieu Williams.

WE require in our houses an artificial temperate climate which shall be uniform throughout, and at the same time we need a gentle movement of air that shall supply the requirements of respiration without any gusts, or draughts, or alternations of temperature. Everybody will admit that these are fundamental *desiderata*, but whoever does so becomes thereby a denouncer of open-grate fire-places, and of every system of heating which is dependent on any kind of stoves with fuel burning in the rooms that are to be inhabited. All such devices concentrate the heat in one part of each room, and demand the admission of cold air from some other part or parts, thereby violating the primary condition of uniform temperature. The usual proceeding effects a specially outrageous violation of this, as I showed in my last paper.

I might have added domestic cleanliness among the *desiderata;* but in the matter of fire-places, the true-born Briton, in spite of his fastidiousness in respect to shirt-collars, &c., is a devoted worshipper of dirt. No matter how elegant his drawing-room, he must defile it with a coal-scuttle, with dirty coals, poker, shovel, and tongs, dirty ash-pit, dirty cinders, ashes, and dust, and he must amuse himself by doing the dirty work of a stoker towards his "cheerful, companionable, pokeable" open fire.

It is evident that in order to completely fulfil the above-named requirements, we must, in winter, supply our model residence with fresh artificially-warmed air, and in summer with fresh cool air. How is this to be done? An approach to a practical solution is afforded by examining what is actually done under circumstances where the ventilation problem presents the greatest possible difficulties, and where, nevertheless, these difficulties have been effectually overcome. Such a case is presented by a deep coal mine. Here we have a little working world, inhabited by men and horses, deep in the bowels of the earth, far away from the air that must be supplied in sufficient quantities, not only to overcome the vitiation due to their own breathing, but also to sweep out the deadly gaseous emanations from the coal itself. Imagine your dwelling-house buried a quarter of a mile of perpendicular depth below the surface of the earth, and its walls giving off suffocating and explosive gases in such quantities that steady and abundant ventilation shall be a matter of life or death, and that in spite of this it is made so far habitable that men who spend half their days there retain robust health and live to green old age, and that horses after remaining there day and night for many months actually improve in condition. Imagine, further, that the house thus ventilated has some hundreds of small, very low-roofed rooms, and a system of passages or corridors with an united length of many miles, and that its inhabitants count by hundreds.

Such dwellings being thus ventilated and rendered habitable for man and beast, it is idle to dispute the practical possibility of supplying fresh air of any given temperature to a mere box of brick or stone, standing in the midst of the atmosphere, and containing but a few passages and apartments.

The problem is solved in the coal-pit by simply and skilfully controlling and directing the natural movements of unequally-heated volumes of air. Complex mechanical devices for forcing the ventilation by means of gigantic fan-wheels, &c., or by steam-jets, have been tried, and are now generally abandoned. An inlet and an outlet are provided, *and no air is allowed to pass inwards or outwards by any other course than that which has been pre-arranged for the purposes of efficient ventilation.* I place especial emphasis on this condition, believing that its systematic violation is the primary cause of the bungling muddle of our domestic ventilation.

Let us suppose that we are going to open a coal-pit to win the coal on a certain estate. We first ascertain the "dip" of the seam, or its deviation from horizontality, and then start at the *lowest* part, not, as some suppose, at that part nearest to the surface. The reason for this is obvious on a little reflection, for if we began at the shallowest part of an ordinary water-bearing stratum we should have to drive down under water, but, by beginning at the lowest part and driving upwards, we can at once form a "sumpf," or bottom receptacle, to receive the drainage, and from which the accumulated water may be pumped. This, however, is only by the way, and not directly connected with our main subject, the ventilation.

In order to secure this, the modern practice is to sink two pits, "a pair" as they are called, side by side, at any convenient distance from each other. If they are deep, it becomes necessary to commence ventilation of the mere shafts themselves in the course of sinking. This is done by driving an air-way—a horizontal tunnel from one to the other, and then establishing an "upcast" in one of them by simply lighting a fire there. This destroys the balance between the two communicating columns of air; the cooler column in the shaft without a fire, being heavier, falls against the lighter column, and pushes it up just as the air is pushed up one leg of an U tube when we pour water down the other. Even in this preliminary work, if the pits are so deep that more than one air-way is driven, it is necessary to stop the upper ways and leave only the lowest open, in order that the ventilation shall not take a short and useless cut, as it does up our fire-place openings.

Let us now suppose that the pair of pits are sunk down to the seam, with a further extension below to form the water sumpf. There are two chief modes of working a coal-seam, the "pillar and stall" and the "long wall," or

more modern system. For present illustration, I select the latter as the simplest in respect to ventilation. This method, as ordinarily worked, consists essentially in first driving roads through the coal from the pits to the outer boundary of the area to be worked, then cutting a cross-road that shall connect these, thereby exposing a "long wall' of coal, which is cut away towards the pits, the roof remaining behind being allowed to fall in.

Let us begin to do this by driving, first of all, two main roads, one from each pit. It is evident that as we proceed in such burrowing, we shall presently find ourselves in a *cul de sac* so far away from the outer air that suffocation is threatened. This will be equally the case with both roads. Let us now drive a cross-cut from the end of each main road, and thus establish a communication from the downcast shaft through its road, then through the drift to the upcast road and pit. But in order that the air shall take this roundabout course, we must close the direct drift that we previously made between the two shafts, or it will proceed by that shorter and easier course. Now, we shall have air throughout both our main roads, and we may drive on further until we are again stopped by approximate suffocation. When this occurs, we make another cross cut, but in order that it may act, we must stop the first one. So we go on until we reach the working, and then the long wall itself becomes the cross communication, and through this working-gallery the air sweeps freely and effectually.

In the above I have only considered the simplest possible elements of the problem. The practical coal-pit in full working has a multitude of intervening passages and "splits," where the main current from the downcast is divided, in order to proceed through the various streets and lanes of the subterranean town as may be required, and these divided currents are finally reunited ere they reach the upcast shaft which casts them all out into the upper air. In a colliery worked on the pillar and stall system—*i.e.*, by taking out the coal so as to leave a series of square chambers with pillars of coal in the middle to support the roof—the windings of the air between the multitude of passages is curiously complex, and its absolute obedience to the commands of the mining engineer proves how completely the most difficult problems of ventilation may be solved when ignorance and prejudice are not permitted to bar the progress of the practical applications of simple scientific principles.

Here the necessity of closing all false outlets is strikingly demonstrated by the mechanism and working of the "stoppings" or partitions that close all unrequired openings. The air in many pits has to travel several miles in order to get from the downcast to the upcast shaft, though they may be but a dozen yards apart. (Formerly the same shaft served both for up and down cast, by making a wooden division (a *brattice*) down the middle. This is now prohibited, on account of serious accidents that have been caused by the fracture of the *brattice*.) But it would not do to carry the coal from the workings to the pit by such sinuous courses. What, then, is done? If any direct road were left open, the air would choose it, but this is prevented by an arrangement similar to that of canal locks and gates. Valve-doors or stoppings are arranged in pairs, and when the hurrier arrives with his *corve*, or pit carriage, one door is opened, the other remaining shut; then the *corve* is hurried into the space between the doors, and the entry-door is closed; now the exit-door is opened, and thus no continuous opening is ever permitted. Only one such opening would derange the ventilation of the whole pit, or of that portion fed by the split thus allowed to escape. It would, in fact, correspond to the action of our open fireplaces in rendering effective ventilation impossible.

The following, from the report of the Lords' Committee on Accidents in Coal Mines, 1849, illustrates the magnitude of the ventilation arrangements then at work. In the Hetton Colliery there were two downcast shafts and one upcast, the former about 12 ft. and the latter 14 ft. diameter. There were three furnaces at the bottom of the upcast, each about 9 ft. wide and about 4 ft. length of grate-bars. The depth of the upcast and one downcast 900 ft., and of the other downcast 1,056 ft. The quantity of air introduced by the action of these furnaces was 168,560 cubic feet per minute, at a cost of about eight tons of coal per day. The rate of motion of the air was 1,097 ft. per minute (above 12 miles per hour). This whole current was divided by splitting into 16 currents of about 11,000 cubic feet each per minute, having, on an average, a course of $4\frac{1}{4}$ miles each. This distance was however, very irregular—the greatest length of a course being $9\frac{1}{10}$ miles; total length 70 miles.

All these magnitudes are greatly increased in coal mines of the present time. As much as 250,000 cubic feet of air per minute are now passed through the shafts of one mine.

The ventilation of our houses may be conducted on the same principles, and with corresponding efficiency, as I will endeavour to show in my next.

FOUND LINKS.

By Dr. Andrew Wilson, F.R.S.E., F.L.S.

PART V.

BACKWARDS in time, and in the course of the geological æons, we find the Cretaceous or Chalk rocks. To the naturalist these deposits have yielded a rich and suggestive harvest of bird-fossils, which, in their approximation to the reptiles, certainly serve as "found links" in more ways than one. In the Chalk rocks of North America we discover the remains of "toothed birds," whose teeth in every respect agree with the structures of that name, and are not mere bony projections, as in the old swimmer of the London clay. The curious *Hesperornis* (Fig. 1) and its neighbours the *Ichthyornis* and *Apatornis*, thanks to the exertions of Professor Marsh, appear before us as veritable links, connecting the birds and reptiles in respect of their teeth, as well as in other features of their economy. Hesperornis stood at least five feet high, and in respect of its bony framework exhibits a close alliance with the grebes of our own day. But strange to say, Hesperornis (Fig. 1) wants one marked peculiarity of other birds (save the ostrich-group), namely, the prominent "keel" or bony ridge on the breast-bone, to which the wing-muscles of birds are attached. The wings were certainly of rudimentary character, but this is a feature we see exemplified in the auks and penguins of our own day; and it is probable that the tail of this great diver of the chalk seas was unusually mobile, and adapted possibly to serve as a rudder. The reptile characters crop out, however, most clearly in the teeth of this bird. There were no teeth in the front of the upper jaw, and presumably this region was covered with a horny beak. The teeth themselves are curved structures; but they are set in a common groove, and not lodged each in a socket, as is commonly the case in higher animals. In living reptiles themselves, it may be added, the teeth, save in crocodiles, are not implanted in sockets. Thus, in serpents and lizards the teeth are simply united by bony union to the bones which bear them; but certain extinct lizards had socket-fastened teeth, and the giant fossil reptiles (*Ichthyosaurus*, &c.) of the Lias, Oolite, and Chalk, possessed teeth

Fig. 1.—Hesperornis regalis.

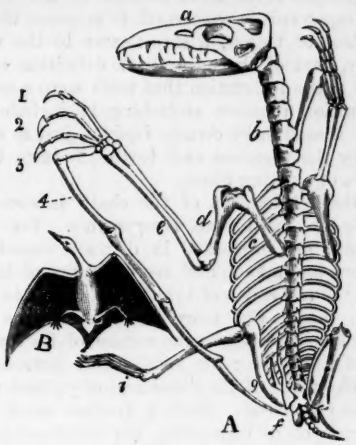

Fig. 2. { A. Skeleton of Pterodactyl. B. Restoration of Pterodactyl.

Fig. 3.—Compsognathus (restored in outline).

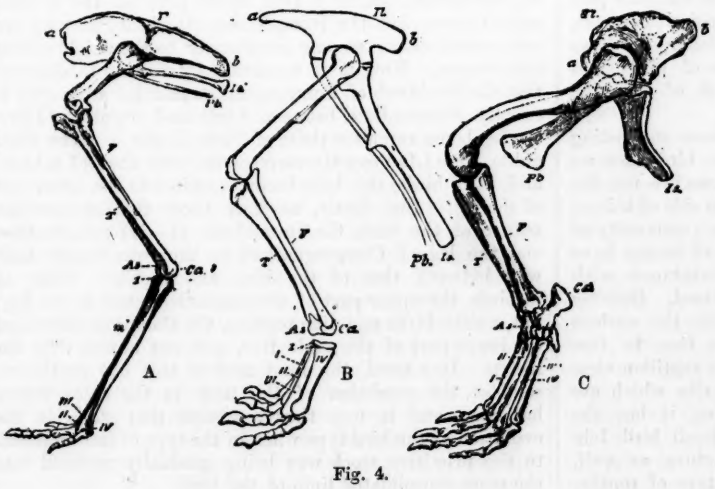

Fig. 4.

Fig. 5. { A. Hind leg of bird. B. Tail bones of bird.

FOUND LINKS.

which likewise arose from sockets in the jaws. In so far as Hesperornis is concerned, it removes the bird class, on the face of things, a step nearer to the reptile hosts. Formerly, part of the naturalist's definition of a bird was included in the declaration that teeth were wanting. Now, the definition requires stretching, to include a character which is shared in by certain reptiles, just as others, represented by the tortoises and turtles, imitate the toothless condition of existing birds.

But the Ichthyornis of the chalk is even a more remarkable bird-fossil than Hesperornis. For the teeth of the former are implanted in distinct sockets, whilst its breastbone had a keel, and its wings are of large size, and indicate the possession of bird-habits, united to structures of reptilian kind. But more peculiar still, as a departure from bird-characters was the nature of its vertebræ or the joints of the spine; for Ichthyornis possessed vertebræ, which, like those of the fishes and of extinct reptiles, were hollow at either end. Such a feature must naturally be made much of in estimating the relationship of this old bird to the reptilian hosts. The size of Ichthyornis was that of a pigeon.

Preceding these birds in time comes the *Archæopteryx* of the Upper Oolite deposits of Solenhofen, in Bavaria. Here the reptile-characters increase in number as becomes the older nature of their possessor. A recently-procured specimen of this bird enabled a zoological authority to declare that it was certainly not wholly a bird, and as certainly not wholly reptile in its nature, but a strict link between these classes. For, firstly, it has the tail of a lizard, that is, the tail is long and jointed, and has no plough-share bone, as in other birds (Fig. 5, B, *b*). Secondly, the bones of its palm were not joined together as in all other birds, whilst at least two of its fingers appear to have been provided with claws, a feature of exact reptilian nature. Then it likewise has been ascertained, by the discovery of the recent specimen already referred to, that this old bird of the Oolite possessed teeth. Judged fairly, then, Archæopteryx is, at the very least, as much a reptile as a bird. Its shoulder and fore-limb (or wing) are decidedly those of a reptile, whilst its hind limbs are bird-like in nature. The facts that such a race of animals once existed, and that they lived at a period when, presumably, the bird-race was undergoing its evolution from the reptilian confines, may, in the eyes of any unprejudiced thinker, serve as clear evidences that the common origin of birds and reptiles is matter, not of speculation, but of scientific demonstration.

I have shown, thus, cursorily, the evidences supporting the contention that if, standing within the bird-class, we look for reptilian features within its limits, we are not disappointed in our search. But on the reptilian side of things, there are also evidences to be found of the community of type from which the birds and reptiles of to-day have sprung. It takes but a slight acquaintance with zoology to discover that the curious lizard, *Hatteria* (or *Sphenodon*), of New Zealand, as befits the curious history of its native country, brings us face to face with characters of abnormal kind, from the reptilian view of matters at least. For this lizard has ribs which are decidedly those of bird-type, and, moreover, it has the same hollow-ended vertebræ seen in the fossil bird Ichthyornis. In other points of its structure as well, Hatteria seems to represent a primitive type of reptile, presumably indicating that stage in the evolution of the two classes wherein certain characters of the bird had already begun to be developed in the common ancestors of these groups.

The flying reptiles (*Pterodactyls*) (Fig. 2) of the Lias, Oolite, and Chalk, teach us that as the pure reptile thus acquired powers of flight, the development of flight in the bird-stock, which was evolved from the reptile, or conjointly with it, need cause us no surprise.

The Pterodactyls possessed the outermost finger (seen in the illustration) enormously elongated, and adapted to form the chief support of a wing-membrane which extended along the sides of the body and between the hind limbs and tail also, as shown in B, Fig. 2. It may be added that these reptiles had a keel on the breastbone like most living and extinct birds, and whilst in some species, the teeth were developed, in others the jaws appear to have been toothless, and to have been sheathed in horn like those of birds. But these reptiles are not "links." They stand, not between birds and reptiles, but at the end of their own side-branch of the great tree of animal life. Still, from the reptile-side, it may lastly be shown that the "found links" connecting them with birds—it may be, of course, in different lines from those indicated by Archæopteryx and its neighbours—already find a place in the geological museum. In those curious reptiles, of which *Compsognathus* (Fig. 3) is the best-known example, the characters of birds and reptiles were united in a literally surprising degree. Imagine a reptile possessing a swan-like neck, with toothed and bird-like jaws; suppose, further, that this animal had very rudimentary front limbs, and that it walked on its two hind limbs like a bird, and we may conceive that this Compsognathus, had it been feathered, would have at least appeared to resemble a bird much more nearly than a reptile. But a still stronger piece of evidence in favour of its bird-relationship awaits the naturalist when he discovers that the hind limbs of these curious reptiles are, in respect of structure, midway and between those of birds and reptiles. If we examine the hind limb of a bird (Fig. 4, A), we notice that the upper half of the ankle (*As. Ca.*) unites with the shin-bone, or leg (T); and as the lower half of the ankle joins the instep (1), the ankle joint thus exists in the middle of the ankle-bones, and the lower ankle and instep-bones form a single bone (*m*) by their union. In Fig. 5 the hind limb of the bird is also seen, *e* being the single bone formed by the union of ankle and instep-bones. In the reptile's limb (C), the ankle-joint, as a rule, opens between the divided ankle-bones; but the instep-bones (C; i., ii., iii., iv.) are not united either to one another, or to the neighbouring ankle-bones. Now it is extremely interesting to discover that the hind-limb of Compsognathus and its allies (B) is exactly intermediate between birds and reptiles. Here, the leg bones resemble those of birds in shape. The chief ankle bone (*As.*) exactly corresponds with that of a bird; and, as in birds, this bone becomes united to the lower end of the leg. But, lastly, as if to show the intermediate nature of the limb, the instep-bone (I—iv) remain free, and the leg of Compsognathus is thus practically halfway between that of the bird and reptile. Thus, as in birds, the upper part of the ankle is united to the leg; but, unlike birds and like reptiles, the Compsognathus had the lower part of the ankle free, and not united with the instep. In a word, the hind limb of this old reptile resembles the condition of the limb in the chick before hatching, and it may thus represent that stage in the evolution of the bird-type wherein the type of limb common to the primitive stock was being gradually modified into the more consolidated limb of the bird.

Thus to-day, there exists a series of forms, detached and isolated, perhaps, but still eloquent enough in their declaration of the existence in past epochs of animals which belong to no one class as defined by us to-day, and which

further stand intermediate between existent classes of living beings. The existence of these "links," to argue backwards, is inexplicable, save on the theory of evolution, or on that of the production of "freaks" by nature; and this last idea, I apprehend, is put out of court, by every consideration worthy the name of a scientific thought.

NOTES ON ROWING.

By an Old Club Captain.

THE race is over, and the despised crews of "inefficient mediocrities" have somehow managed to take their boats over the course from Putney to Mortlake (on a good, but not wonderfully good, tide), in the very good times of 20 min. 12 sec. and 20 min. 32 sec. for winners and losers respectively. I was able to watch both crews under singularly favourable conditions as they rowed past the White Hart, at Mortlake. With an excellent field-glass by Browning, I had each crew in succession about half a minute in view, as distinctly as if they were at an oar's length from me. For a minute or two before and after this, I was able to see both crews together, foreshortened, so that the nature of the swing, and the time of stroke and recovery could not only be well seen, but readily compared. [I had before only seen the crews in practice, and always at the beginning of their spins. Oxford, in particular, I had seen under very favourable conditions.] At Mortlake on Saturday, there was a difference naturally arising from the circumstance that Oxford were rowing out to the finish a race already won, while Cambridge—though they could not be so sure of the hopelessness of their position as the Oxford men were sure of victory—were nevertheless very obviously rowing a losing race. There was not seen that liveliness on the feather, after rather sluggish disengagement of the oar, which had been characteristic of the Cambridge style. On the other hand, the Oxford men showed the good features of their style very markedly. The swoop down of the oars upon the water was splendid at this stage of the race (I am told that earlier there was occasionally some little trace of flurry). As for the way in which the work was done—a question which I left not quite decided in my last—I am as certain, after my observations last Saturday, as I am of my own existence, that nearly all the arm work was done in conjunction with body and leg work, and not, as some persistently assert, afterwards. I am equally certain that in the Cambridge boat the arm work followed the sway back of the body. The sluggish look of the Cambridge style was in singular contrast to the sharp slash of the Oxford oars through the water; and this sluggish look was entirely due to the inert condition of the arms during the first part of the stroke. The slow disengagement of the oar by the Cambridge, again, was in decided contrast with the clean, quick disengagement by Oxford. On the other hand, I cannot say that the swing forward by Cambridge was anything like so sharp as I had expected to find it, or as it appeared in practice. But this was only natural in a crew which had been rowing so hard a stern race. Moreover, the Cambridge style is, as every one knows who has ever tried it, exceedingly wearing in a hard race; and the lightning feather, which is its one theoretical advantage (more than counterbalanced by disadvantages), is very apt to be exchanged towards the end of a long race for a much less lively movement. This circumstance, which I have noticed myself in rowing races (I suppose every reader of these lines has already come to the conclusion that the writer is a Cantab) is well described by Mat Bradwood, in a passage which deserves to be quoted at full length, so apt and instructive is it. "Every day of practice on the Cam," he says, "you hear the coaches of the different racing-boats giving their crews certain directions, some absurd, and nearly all from some accidental reason useless. The chief of these is to 'keep it long,' and if you object to the results of this teaching, you are told that 'length' is the great requisite of good rowing, and that 'Oxford, sir, always beat us, because they are longer than we are.' Now this is true and yet untrue. At Cambridge, 'length' is acquired by making the men 'finish the stroke,' that is by making them swing well back beyond the perpendicular. Of course the oar remains longer in the water, but we maintain that the extra time it is kept there by the backward motion of the body is time lost. The 'swinging back' throws a tremendous strain on the abdominal muscles, the weakest rowing muscles in the body; very soon the men feel this strain, become exhausted and unable to 'get forward,' and, finally, lose time and swing and 'go all to pieces.' Length obtained by going backwards is of no possible use. A crew ought to be coached to go as far *forward* as they can, to finish the stroke by bringing their elbows past their sides, and their hands well in to their bodies, and then complaints about 'wind' and 'last' will be fewer."

It may be asked why, if the repeated victories and the nature of the victories of Oxford result from an inherent superiority of the Oxford over the Cambridge style, Cambridge does not adopt the Oxford style? It is well known that Cambridge club captains try to introduce what they believe to be the style of Oxford. The passage above quoted indicates very accurately the feeling of Cambridge men on this point. But a radically wrong idea is entertained at Cambridge as to what the Oxford style really is. Oxford men are apt, as I have already mentioned, to assert (and doubtless they believe, though erroneously, as close observation and theory alike show) that they do not use the arms till the body is nearly upright. Thus, Mr. Skey, F.R.C.S., in writing on the movement of the muscles and the body in rowing, gives as the result of inquiry which he had made, the following entirely incorrect account of Oxford rowing:—"The prominent and distinctive feature of the Oxford system consists, I believe, in this that the action of the glutei (the great muscles of the buttock), in drawing the trunk backwards to something beyond the vertical, is nearly exhausted before the agents of flexion of the forearm commence their work. The Oxford authorities consider that they row with their trunk, while others more prominently row with their arms. In truth" (here his anatomical knowledge sets Mr. Skey right) "the muscular system of both trunk and arms is indispensable in all cases, the only distinction being that in the case of Oxford oarsmen the greater part of the retraction of the trunk, by the action of the glutei is accomplished with rigid unbent arms, while in other cases the retractors of the shoulders, and the flexors of the forearm are somewhat more in unison, or rather, they share the time occupied by the former action." Mr. Skey is "unwilling to express a positive opinion as to the relative excellence of the two styles in rowing," but is inclined to think that some advantage is obtainable from the two actions being rendered consecutive, inasmuch as the superior power of the retractors of the trunk, on which the great effort in rowing depends, should be exerted singly, without the physical action of the system being hampered by two actions at the same moment of time." As this was written in October, 1869, when Oxford had been for nine successive years successful against Cam-

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bridge, and an Oxford four had beaten in hollow fashion the American four from Harvard, it was only natural that Mr. Skey should be inclined to think the Oxford style must be the better; and so it unquestionably is; but the style he describes is that which was, and is, enjoined rigidly by Cambridge captains, and observation shows that Oxford does not follow that style, but brings the arms to work from the beginning. How any doubt could ever be entertained on this point by those who know how much farther back than Oxford the Cambridge crews have always swung, is difficult to understand. For, if the arms are straight when the body reaches the vertical position, the body must, of course, swing further back while the arms do their work, as yet scarce begun; but, if the arms have been at work while the body is swinging to uprightness, the arms have little left to do, and therefore the body need swing back but a very little. As Oxford men unquestionably do *not* swing far back, while Cambridge men do—even men who know nothing practically of rowing can see *that*—it should need no demonstration that Oxford men do more of their arm-work than Cambridge men during the beginning of their stroke. "And I myself, with my very own eyes have seen," as the old chronicler wrote, that Oxford men and London men too, and watermen always, row with body and arms, and legs too, from the very beginning of the stroke.

Meanwhile, at Cambridge they quietly accept the faith that Oxford men swing back with rigid, unbent arms (and they see this in tub practice and paddling, where it is the correct thing to do); so they keep to the rule, unconscious or forgetful that it is the old rule, good only for the old style of boats; and so they get beaten, unless they have overwhelming superiority of strength, when they win by half a length or a length, where Oxford, with the same superiority of strength, would have won by half-a-dozen or a dozen lengths.

I write confidently, but I write when there is good prospect that the matter will be tested in a way there can be no mistaking (I would stake a good deal on the result if I were a betting man). Mr. Muybridge's method of instantaneous photography, which has shown how a horse gallops and how an athlete turns a somersault, will very readily show how an Oxford or a Cambridge oarsman rows, and is very likely soon to be applied to the work. But there is a practical test which Cambridge captains might very easily try, and perhaps apply the result in a way very pleasing to themselves and their crews. Let a crew, each member of which rows well the Cambridge dragging swing-back, go over a measured distance, say half-a-mile, at their best speed in that style, and then let them—though, perhaps, a little tired—go back to the starting-place and go over the course again with a changed style. Let stroke and each man of the crew agree for awhile to give up their cherished drag and lightning feather; let the arms be sturdily called into action, in due subordination, of course, to body and in due alliance with the legs, from the very beginning of the stroke, so that when the body comes upright, the arms have nearly done their work. Let not the stroke be hurried, but a steady (not sluggish) recovery, precede the simultaneous grip at the beginning. I will undertake to say that, even at the first trial (absolutely important though practice is in this style, where everything depends on the work being done at the same time), even at the first trial, I say, the result will be such as to show unmistakably how much more effective, and also how much easier, this style is than the style inculcated forty years ago for boats as different from those of our time as a barge from a pleasure-boat, or a pleasure-boat (with a party of ten under her awning) from the old clinker-built racing craft.

THE ELECTRICAL EXHIBITION AT THE CRYSTAL PALACE.

Eighth Notice.

IT is not difficult to understand that if a certain amount of electric energy is required to render a carbon filament 1 in. long incandescent, ten times that energy will be required (presuming there is no other resistance in the circuit) to similarly heat a filament 10 in. long. If, however, we have sufficient energy to heat 10 in. of carbon, it matters little whether that carbon is in one piece or in ten, or in any number of pieces making up the same total length. Here, then, is the first principle in the so-called division of the electric light. The great object to be achieved is really a *distribution* of the light, necessitated by the great diminution of luminosity as we recede from the source of light. The exact falling-off is "inversely as the square of the distance," that is to say, if we have a luminosity equal to twenty candles at a distance of 1 ft., the intensity at 2 ft. will be—

$$2^2 : 1^2 :: 20 : x$$
$$\text{or } 4 : 1 :: 20 : 5$$

5-candle power will, therefore, be the luminosity at 2 ft. distance. This explains how it is that a large light is so expensive, or lacks economy so much, when compared with a number of small lights placed at the proper distance from each other to illuminate the same area.

Fig. 1.

Fig. 1 illustrates the method of connecting the lamps, known as "joining in series"—that is to say, the whole of the current passes through each lamp in succession.

Fig. 2 illustrates another method of joining up, and is known as the "multiple arc," or "quantity" arrangement. The current comes in from the machine at +, and returns by the other wire marked −. In this case the current, instead of passing through each lamp in succession and having to overcome the added resistance of, say four lamps, is assisted by their conductivity, and divides itself between them, the resistance being thereby considerably reduced, just as a flow of water which requires a certain pressure to push it through, say, four yards of pipe, requires one-fourth of that force to drive it through one yard. If we were to place four pipes, one yard long, side by side, the required pressure would be again quartered. Actually, therefore (assuming each lamp to offer 50 ohms' resistance), joining in series would give 200 ohms for four lamps, while in multiple arc the resistance would be reduced to 12·5 ohms.

But suppose again that in the single pipe the quantity of water was only sufficient to fill one pipe, we should have to quadruple the supply of water in order to fill four pipes placed side by side. Accordingly, the current required for the arrangement described is of less intensity, but of greater volume, to produce equal degrees of luminosity.

Fig. 3 is another arrangement known as "mixed," that is, a combination of series and quantity. The diagram may be left to explain itself.

We should have liked this week to give, in continuation of our remarks on incandescent lighting, a few mathematical details concerning the arrangement of the lamps and generators. These details, however, will be best comprehended after we have finished our description, in

another series of articles, of the fundamental principles of electric generators.

The incandescent light is, beyond doubt, the light of the future, and we have no hesitation in saying that before long we believe it will be proved as much cheaper as it is brighter, purer, and healthier than gas. At the same time we must not lose sight of the fact that it ensures perfect immunity from all those possible catastrophes inherent to gas. An incandescent lamp, even when broken, is perfectly harmless, and it has been declared that it could be broken in the centre of a barrel of gunpowder without the slightest danger. The reason of this is that the moment the glass is broken, or even cracked, air rushes in to occupy the previously vacuous globe, and coming in contact with the carbon filament, oxidation instantaneously takes place, the filament being thus a continuous conductor no longer. The only danger is when the covering of the wires gets

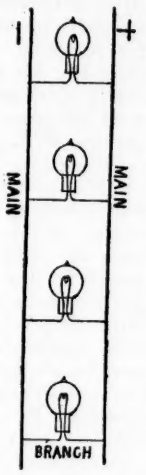


Fig. 2.

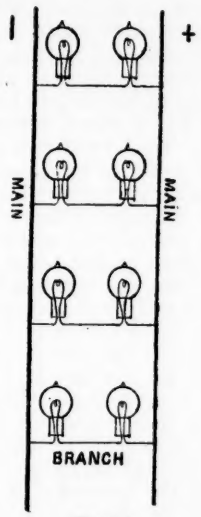


Fig. 3.

damaged, or in any other way allows the current to pass from one wire to the other without going through the lamps; then great heat would be developed, sufficient, perhaps, to set fire to the building. This danger, however, is easily guarded against by inserting a piece of easily fusible metal, such as lead, in the circuit, which, on getting hot, melts and breaks the circuit, the only inconvenience being the extinction of all the lights in that section, until the piece of lead is replaced. And what is this inconvenience compared to what would result from a gas explosion?

Anent the various systems now before the public, there is not much to choose between them, but for brilliancy and durability the Swan certainly leads the way. The Electrolier in the Furniture Court is simply beautiful, and contrasts in the delicate outline of its design with the gorgeous display of Mr. Edison. Nor must we forget Messrs. Elkingtons' show-room, which is very tastily adorned by the Swan light. The Maxim and Lane-Fox systems are both very good, and try hard to outdo each other in their application to delicate glass-work.

Explosive Paint.—It is stated that the Admiralty have directed that the whole of the xerotine siccative in store at the various dockyards should be immediately destroyed.

THE GREAT PYRAMID.

By the Editor.

THIS week my remarks upon the Pyramid must be brief, for the present number (on account of the approach of Easter week) appears under certain disadvantages. I take the opportunity of noting sundry objections to my views, which have been suggested by certain readers.

In the first place, many seem quite unaware of the difficulty of orienting a building like the Great Pyramid with the degree of accuracy with which that building actually has been oriented. One gravely asks whether (as Narrien long since suggested) a plumb-line, so hung as to be brought into line with the Pole Star, would not have served as well as the great descending passage. Observe how all the real difficulties of the problem are overlooked in this ingenious solution. We want to get a long line—a line at least 200 yards long—in a north and south position. We must fix its two ends, and as the pole-star is not available as a point along the line, we set our plumb-line at the northern end of the line, and our observing tube or hole, or whatever it may be (only it is not a telescope, for we are Egyptians of the time of Cheops, and have none), at the other. The pole-star being at an altitude of $26\frac{1}{2}$ degrees, the plumb-line should be nearly 100 yards long, to be seen (near the top), coincident with the pole-star, from a station 200 yards away. That is a tolerably long plumb-line. Then its upper part (thus to be seen *without telescopic aid at night*) would be about 260 yards away. The observer's eye-sight would have to be tolerably keen.

I am also asked whether a dishful of water would not serve quite as well as a great mass of water, at the corner where the descending and ascending passages meet, to give the reflected rays from a star. It would, and so would a thimbleful—just as a thread of cotton would serve as well as a half-inch rope for the plumb-line just considered. But just in proportion as the water surface was diminished would the difficulty of seeing a star by reflected rays be increased. The builders had, doubtless, good reason for making the descending passage about 4 ft. wide and as many high. It at any rate enabled them to see the pole-star readily, just as the wide field of view of a comet-finder enables the astronomer to bring a celestial object very easily into view. Whatever reason they had for thus securing a tolerably large field of view, they would have precisely the same reason for retaining it undiminished when they used the reflected instead of the direct rays, in observing a star. Now for this purpose nothing short of the whole breadth of the descending and ascending passages, would suffice—in other words, no dishful or thimbleful of water would have served their purpose.

Then the *Saturday Review* asks why the descending passage should be repeated in the other pyramids when the orientation had already been secured in the Great Pyramid—manifestly quite ignorant of the fact that it would be far more difficult to take the orientation for one pyramid from another, than to do it independently. It also asks whether the slant descending passages were not obviously meant for the sliding-down of the King's sarcophagus. Sliding the sarcophagus down that it might afterwards be hauled up the ascending passage! or if not, what was the ascending passage for? and why was it of the same cross section as the descending passage? If the sarcophagus alone had been in question, we may be certain that the pyramid engineers would never have arranged for sliding it down from the level of the entrance to the descending passage, to the place where the ascending passage begins, in order afterwards to raise it by the ascending

passage. If they meant to go down to the underground chamber they would not have raised it at all, but let it down from the level of the pyramid's base. But, to say truth, moving the sarcophagus was a mere nothing compared with the lifting of the great solid blocks which formed the pyramid's mass. The engineers who moved these great blocks to their places, would not have wanted slant passages at the right friction slope, and all the rest of it, by which to take the sarcophagus to its place; nor would they have provided for unnecessary descents or ascents either, but have taken the sarcophagus from the outside to its proper level, and sent it along a long level passage.

The *Saturday Review* says further—but, what *can* it matter what the *Saturday Review* says on subjects such as these?

A correspondent, Mr. J. F. Hodgetts, touches on the association which I mentioned as existing between the Jewish Sabbath (our Saturday) and Saturn; labouring, manifestly, under the impression that the point at issue was the identity of the Roman god Saturn with the Scandinavian deity assigned to Saturday. But of course he has entirely misapprehended me. It is not the god Saturn, but the planet Saturn, which is associated with Saturday. How Mr. Hodgetts can reconcile the clear statement of Dion Cassius with his belief that the days of the week were not associated with the planets until the twelfth century, passes my comprehension. Dion Cassius distinctly attributes the invention of the week to the Egyptians, and as he wrote a thousand years before the time named, there must be something wrong in Mr. Hodgett's dates. In the ancient Brahminical astronomy the days are associated with the same planets as among the Egyptians. See Mr. Colebrooke's papers in the "Asiatic Researches." Among more familiar discussions of this matter may be cited Bailly's "Astronomie Indienne et Orientale," and Bohlen's "Das Alte Indien."

Dion Cassius refers to the connection between musical intervals and the planets, showing that probably the old Egyptian lore which Pythagoras of Samos brought to Greece, included the association between the planets and the days of the week; that, in fact, all three subjects were connected—planets, musical intervals, and the days of the week. Longfellow thus poetically renders the views of Egyptian astrologers on these, with them, mystical matters:—

> Like the astrologers of eld,
> In that great vision I beheld
> Greater and deeper mysteries.
> I saw, with its celestial keys,
> Its chords of air, its frets of fire,
> The Samian's great Æolian lyre,
> Rising through all its sevenfold bars,
> From earth unto the fixed stars.
> And through the dewy atmosphere,
> Not only could I see but hear
> Its wondrous and harmonious strings
> In sweet vibration, sphere by sphere;
> From Dian's circle light and near,
> Onward to vaster and wider rings,
> Where, chanting through his beard of snows
> Majestic, mournful Saturn goes,
> And down the sunless realms of space
> Reverberates the thunder of his bass.

Errata.—In the letter-press accompanying chart of the path of Mars, second column, line 10, for "south to north" read "north to south," and line 12, for "north to south," read "south to north." Line 15 for "plane of his orbit," read "plane of the earth's orbit." The mistakes could scarcely deceive the carefu lreader but it is as well that they should be corrected.

PHOTOGRAPHY FOR AMATEURS.

By A. Brothers, F.R.A.S.

PART III.

BEFORE leaving the historical part of the subject, it may be interesting to refer to the partnership which was entered into between Niepce and Daguerre. The one appears to have rendered no assistance whatever to the other—and to Daguerre alone is due the credit of working out the process which was considered by the French Government of so much importance, that Daguerre was awarded a pension for life of £240 a-year, and to Isidore Niepce* £160 a-year—the half to revert to their widows. Considering the great value of the discovery, the sum awarded was ridiculously small; but, although the French Government appear to have presented the discovery to the world, Daguerre patented the process in England, and must have realised a large sum by the sale of licenses.

For many years the two processes of Talbot and Daguerre were extensively used. The Talbotype, or Calotype, as it was indifferently called, was more suitable for landscapes and architectural subjects than for portraits, and the Daguerreotype was used almost universally for portraits, and may be said to have been without a rival until 1851, when Mr. F. Scott Archer presented his process to the public. Sir John Herschel, Mr. Bingham, M. Legray, and others, had tried various means for utilising glass on which to produce the negative image; but to Mr. Archer, assisted by Dr. Diamond, is due the honour of inventing a process which produced the most perfect results. Mr. Archer was a sculptor; he died a few years after the publication of his process, and as it then became known he was far from being in prosperous circumstances, his generosity in seeking no pecuniary advantage in the sale of his process, or by securing it by patent, will be fully appreciated. Mr. Archer did not invent the substance known as *collodion*, which consists of gun-cotton dissolved in ether and alcohol; but he made it available for photography. By adding an iodide or bromide, or both, to the collodion, and then, when the film had *set*, by immersion in a solution of nitrate of silver, the conversion of the iodide or bromide of ammonia (or other similar salt) into an *iodide* or *bromide of silver*, the collodion film is rendered very sensitive to light. The collodion process is in universal use, and for thirty years may be said to have had no rival. Latterly, collodion has, to some extent, been superseded by gelatine. We shall see wherein the processes differ in a future paper. We have now given the main facts relating to the history of photography. If space had permitted, we might have entered into the subject with more detail. The merest glance has been given, and it has been a matter of some difficulty to select the main facts. A volume could be filled with the interesting details, while we have attempted to tell the story in a page or two.

The art of photography, as at present practised, is of great simplicity; but, easy as it is, some experience is necessary, and many failures may be expected before the amateur can look with satisfaction on his work. His first attempt *may be* a perfect success, and so perhaps may his second be, but his third attempt may be as great a failure, and from want of experience he will be altogether unable to account for his want of success. Much may often be learnt from failures, and for the satisfaction of the beginner he may be informed that failures frequently occur in the hands of those who have practised photography for many years. No rule can be laid down for accounting for

* Nicéphore Niepce died before the pension was granted, but the partnership was continued with his son Isidore.

failures—the causes are so various, that they require in each case to be traced to their source, and not infrequently the cause will not be discovered. The amateur, therefore, must not be discouraged by failures.

Advice will naturally be sought as to the outfit required; and the reply to this is that if expense be no object, the best apparatus procurable should be purchased at the outset. Our advice is, determine what you wish to do, and then go to a dealer in photographic materials and apparatus, and he will supply a list of what is necessary. If pictures not larger than 5×4 will satisfy the ambition of the beginner, the expense of the outfit will not be great—£5 will supply camera, lens, and chemicals sufficient to start him in his new and, as he will soon find, fascinating hobby. Perhaps the most useful size for the camera for the tyro is one for taking pictures 7×5, and if a lens of the "rapid rectilinear" form be adopted, portraits as well as landscapes may be taken with it, and it will not be necessary to have a special portrait lens for that class of work, for the amateur will soon find that portraiture is not the least pleasing of the uses to which he may put his apparatus.

It is assumed that all the necessary apparatus and chemicals have been obtained, and we must now describe how they are to be used; and it is assumed, also, that the amateur will commence with the wet collodion process. Having become expert with this, he will have comparatively little difficulty in practising the newer and quicker process with gelatine dry plates, which will be described later. First of all, it is necessary that the glass used should be of good quality and perfectly clean. Patent plate-glass is, of course, to be preferred; but any glass of good quality will answer. The glass must be perfectly clean; and, to effect this, whiting or Tripoli powder may be made into a thin paste with alcohol and water—say equal parts. After the glass has been washed in clean water, a small quantity of the Tripoli paste may be dropped on to the glass, then rubbed over *both sides* of the glass, and then rinsed off under a tap of running water. The cloths used for drying the plate should not be used for any other purpose, and should be washed without soap. When dry, the plates should be polished with a clean leather, kept strictly for the purpose. If breathed on, it will be seen at once whether the glass is perfectly clean. Before coating the plate with collodion, all dust should be brushed off with a large, soft brush, kept for the purpose.

BRAIN TROUBLES.

Irritability.

AMONG the most characteristic signs of mental weariness, irritability may be mentioned. We use the word rather in its technical than in its ordinary sense. Nervous irritation may be indicated quite as much by gloom and melancholy, as by temper or impatience. When we find ourselves disposed to take unreasonably gloomy or unreasonably fretful views of our affairs, to be troubled or vexed (that is, made sorry or angry) by trifling matters, we may be assured that there is something wrong with us. The mischief may be bodily, or it may arise from external causes; but usually—at any rate with those who exercise the mind more actively than the body—the cause of the change is mental. It is not always easy to distinguish between these various forms of irritability. Those who are affected by the east wind can ascertain, when they find themselves out of sorts, whether the wind is easterly or not; but probable that the mere liabilit be thus affected is a sign of nervous weakness, which may result from mental overwork.* And there are some meteorological causes of irritability not so easily inquired into as the influence of an easterly wind. (Has it been commonly noticed, or is the experience exceptional in the writer's case, that when the mind has been heavily taxed, blusterous weather produces the effects usually attributed to easterly winds?)

Again: some of the forms of irritability due to bodily mischief are not easily distinguished from those due to mental overwork. Thus, a case is related of a young man noted for his gentleness, who, forming one evening a member of a brilliant party (his companions being of his own age), was quarrelsome and cross-grained, wrangling with, and in the end offending, everybody in the room. Two hours after he was seized with nephritic torments, caused by a calculus, which did not cease to trouble him till the next day. The writer can recall an even more striking case of the sort in his own experience. He had been struck by his own exceeding ill-temper (which, utterly wrong-headed though it seemed, he felt quite unable to control), while visiting, at the request of several of the professors of Yale College, the laboratories and technological collections of that institution. He could in no way distinguish his irritability from that which he had learned to regard as the effect of over-work. But it continued (though he had had and availed himself of an opportunity for resting) for more than twenty-four hours. Soon after (for there was an interval during which the sense of ill-temper and despondency passed away), he was attacked by renal tortures, which, unlike those of the amiable young man of the previous story, lasted more than a week, and amply justified (in the writer's opinion) all the ill-temper he had displayed beforehand,—if at least the disorder of the nervous system before the attack could be measured by the intensity of the pains suffered during the attack.

Usually, however, an indefinable feeling of irritability and ill-temper signifies that the mind has been overworked. So, also, does that state in which, to use a commonplace but convenient expression, everything seems to go wrong. In reality, we do everything wrong, though we may be unable to recognise any difference between our way of attending to those slight matters on which the pleasant progress of our work depends and our customary methods. We misplace this and upset that, tear, smear, blot, and so forth, not because the fates are for the time being against us, but because we are weary and overwrought (though we may not be conscious of it), and our hands and fingers are not under the usual control of the mind and will.

* Dr. Forbes Winslow describes a curious instance of morbid irritability of this kind. "A military man, suffering from severe mental dejection, was in the habit," he tells us, "of promenading backward and forward in a certain track, towards evening, on the rampart of the town in which he resided. When he walked forward, his face fronted the east, where the sky was hung with black, as was, alas! his poor soul. Then his grief pressed doubly and heavily upon him; he was hopeless and in deep despair. But when he turned with his countenance towards the west, where the setting sun left behind a golden stream of light, his happiness returned. Thus he walked backward and forward, with and without hope, alternating between joy and melancholy, ecstasy and grief, in obedience to the baleful and benign influence[s] of the eastern and western sky!" Alfieri says, in his "Memoirs," "I have observed, by applying to my intellect an excellent barometer, that I had greater or less genius or capacity for composition according to the greater or less weight of the atmosphere: a total stupidity during the solstitial and equinoctial winds; an infinitely less perspicacity in the evening than in the morning; and much more fancy, enthusiasm, and invention in midsummer than in the intervening months."

XUM

DOES THE LUMINOUS MIXTURE OF BLUE AND YELLOW MAKE GREEN LIGHT?

By Lieut.-Colonel W. A. Ross, late R.A.

PROFESSOR HELMHOLTZ says in a lecture on "The Recent Progress of the Theory of Vision" (Second English edition, Longmans, 1881), "It is impossible to make a green out of blue and yellow light." He supports this strong statement by the following argument. "The simplest way of mixing* coloured light," is by the observer looking at a blue (or yellow) wafer placed on a table, through a small flat piece of glass, so fixed as to also receive the reflected image of another yellow (or blue) wafer a little distance away. If *b* is the wafer from which rays are transmitted through the glass, and *g*, that from which rays are reflected, and if they are properly placed, "it then appears as if there was a single wafer at *b*, with a colour produced by the mixture of the two real ones. In this experiment, the light from *b* which traverses the glass pane, actually unites with that from *g* which is reflected from it, and the two combined pass on to the retina." In this way Prof. Helmholtz finds that "the union of blue and yellow rays of light produces white."

Similarly, Prof. Rood says ("Modern Chromatics," 1879, p. 109) "Brewster's theory of the existence of three fundamental kinds of light—red, yellow, and blue—is found in all except the most recent text-books on physics, and is almost universally believed by artists. Nevertheless, it will not be difficult to show that it is quite without foundation." Eliminating a rather silly observation, that the theory "cannot be true, because there is no such thing as colour, which is a mere sensation varying with the length of the wave producing it,"—on which basis of "argument" we might ask why Prof. Rood took the trouble to write a book about mixing colours, when "there are no such things"—the "rude" assertion is found to be supported by evidence derived from the same kind of experiment as that of Helmholtz (devised by Lambert) above quoted.

Now, I do not take upon myself for a moment to assert that the "theory" of blue and yellow lights together forming white may not be correct; all I respectfully maintain is, that facts, so far as I have been able to produce them, seem to point in the other, or Brewsterian, direction.

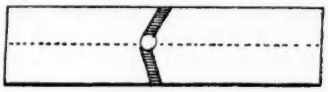
Fact 1. If we make a paste with a drop of distilled water, a little powdered *Atacamite* (or pure chloride of copper) and a certain proportion, which can be easily ascertained by experiment, of any sodium salt, and burn this paste on a platinum wire before a blowpipe, we obtain a beautiful deep green flame, or coloured light; the green colour being evidently due to the mixture of the blue light from the burning *Atacamite*, and the yellow light from the burning soda.

Fact 2. Bend about a quarter of an inch of thickish, clean, new platinum wire at a right-angle to the rest of the wire or shank, and hold it before the blowpipe, so that the yellow flame (afforded by all platinum wires in such a position until they become white hot) shall combine in a certain proportion with the blue, blowpipe-pyrocone, and you immediately obtain a distinct, though not very "strong,"—from the minute quantities of the ingredients used—green light. I ascertained this fact some years ago from some platinum wire given to me by Messrs. Griffin & Co. to examine, as they supposed, from this "green flame," it contained copper; but I showed them that, not only their wire, but everybody's, could, by the above-mentioned manipulation, be made to yield a "green flame."

Fact 3. Chemists are aware that, by turning a gas—"Bunsen"—very low, the blue pyrocone suddenly becomes green, just before the little explosion takes place which extinguishes it. The cause of this green colour is soon ascertained to be the burning of a certain proportion of "sodium" (contained in the air admitted) to the blue Bunsen-pyrocone; for, if we admit an excess of air to the "Bunsen" in full power, we find the blue "flame" almost completely yellowed. There are other ways of producing a green light from the mixture of blue and yellow lights by direct combustion; as, for instance, by introducing a rather moist piece of wood into a coal fire "burning blue," as a friend and I deliberately effected the other night at his house in Phillimore-gardens; but the above will suffice, so far, to disprove Helmholtz's assertion.

Fact 4. Let us now focus, in a semi-darkened room with open window, but venetian blind down, a sunbeam admitted through an upturned lath, by means of a biggish lens (I used the object-glass of a lorgnette of 2 in. diameter, when I discovered the fact last October) upon the face of a glass prism held perpendicular to the beam. Instead of a spectrum on the wall, which the beam, without the interposition of the lens, occasions, we find the sunbeam continued within the prism, but of a brilliant grass green colour; although bubbles and other objects in the path of the green beam through the glass, reflect white light. This curious effect seems to me to be the result of the elimination of the least refrangible, or red rays, from the beam in its passage through the glass (for any-shaped, if moderately deep, piece of glass will do, but a mirror will not do), because, from their less elasticity when thus concentrated by the lens on the face of the refracting medium, they refuse to be bent like the other more refrangible rays (blue and yellow), and thus take up a different path from those; *i.e.*, are reflected and dispersed. The blue and yellow rays, left to pursue their path through the glass alone, combine to form green.

Your knowledge of mathematical optics will enable you to determine whether my ignorance of that subject is causing me to "talk nonsense" or not; but the above is the only explanation which occurred to me of this remarkable phenomenon when I observed it last October. Anyhow, the fact is, I believe, quite a new fact in optics; and, as the same phenomenon may occur whenever solar rays are collected by a lens for delivery upon a prism, as in most spectroscopes, although not with sufficient intensity to make it visible, as in the case of focussing, it would be very interesting to ascertain, by some optical analyzing process, whether the fact may not have affected spectroscopic results as now received?



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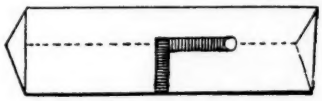
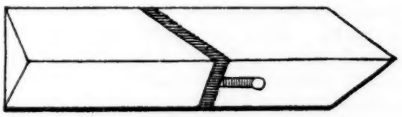
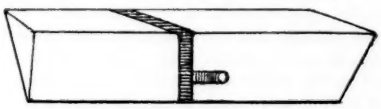


From Right Front.



From Top and Right Front.



Viewed from Top.

From Front.

In these rough figures, the small white circle represents the focussed sunbeam impinging upon the face of a glass prism in various positions. The shaded lines show the path of the beam, now changed to a green colour, within the prism. The dotted lines are intended to represent those sides of the prism which are seen *through* it.

A common glass globular paper-weight is an excellent medium for the exhibition of this phenomenon.

* Query: Does this process really "mix" lights? I should like to see this proved.

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Pond's Extract is a certain cure for Hæmorrhoids.
Pond's Extract is a certain cure for Neuralgic pains.
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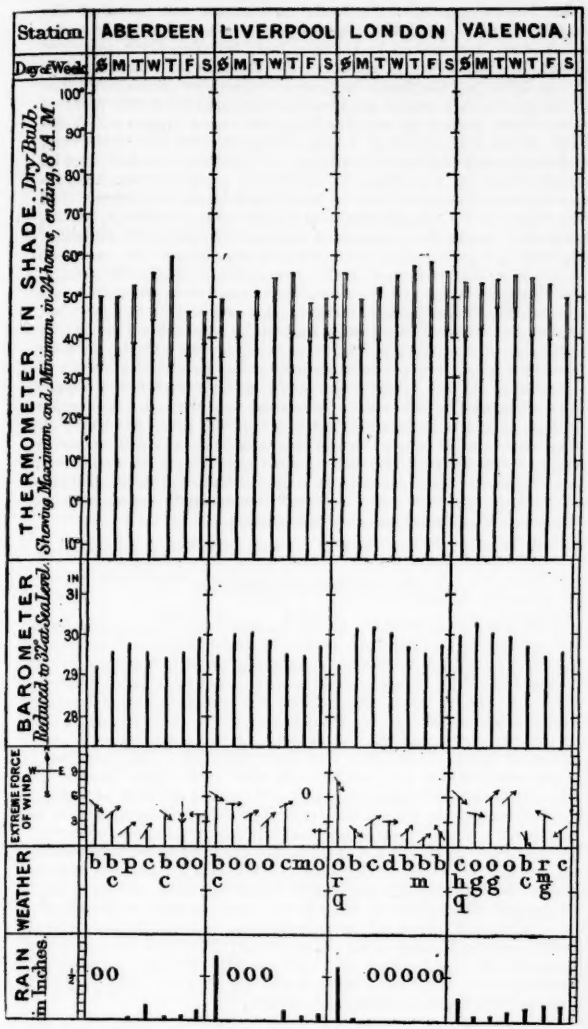
WEATHER DIAGRAM.

FOR WEEK ENDING SATURDAY, APRIL 1.


Station
ABERDEEN
LIVERPOOL
LONDON
VALENCIA
Days of Week
S M T W T F S
THERMOMETER IN SHADE. Dry Bulb. Showing Maximum and Minimum in 24 hours, ending 8 A.M.
BAROMETER Reduced to Sea Level.
EXTREME FORCE OF WIND
WEATHER
RAIN in Inches.
b b p c b o o b o o o c m o o b c d b b b c o o o b r c c c c h g g c m g q r q m q
0 0 0 0 0 0 0 0

Weather.—*Beaufort Scale* is, b. blue sky; c. detached clouds; d. drizzling rain; f. fog; g. dark, gloomy; h. hail; l. lightning; m. misty (hazy); o. overcast; p. passing showers; q. squally; r. rain; s. snow; t. thunder; u. ugly, threatening; v. visibility, unusual transparency; w. dew.

AMALGAMS.

OPINION is still divided with regard to the nature of amalgams, some considering them to be isomorphous mechanical mixtures, others true chemical compounds. The former view derives support from those cases in which amalgamation is associated with an absorption of heat, as in the solution of a salt or in dilution of a solution; the latter is supported by the fact that many amalgams are formed with a strong development of heat. A contribution to the subject has been lately made by Herren Merz and Weith, in the Berlin Chemical Society. These chemists have investigated whether, with regular heating, amalgams part with their mercury continuously or in distinct gradations.

The experiments consisted in placing the amalgam in a porcelain dish within a glass tube, contracted below, and inclosed in a second tube, having a bulb at its lower end. This bulb of the outer tube contained the substance of the vapour-bath (sulphur, mercury, or diphenylamine). To guard the amalgam from air, a lively current of an indifferent gas was passed through the interior tube while the experiment lasted. The amalgams used, which were always directly produced by known methods, contained on an average 60 to 80 per cent. of mercury. This heating was continued, wherever possible, until after several hours no decrease of weight (or hardly any) was perceptible. There were examined gold, silver, copper, lead, tin, bismuth, zinc, cadmium, sodium, and potassium amalgams. The results for the first eight are very briefly communicated, those for the last two, whose easy oxidability required special precautions, more fully. In the case of these alkali amalgams, the authors also sought to determine the melting points, but, for certain reasons, very accurate results were not reached. In general the melting points of the amalgams rise at first very quickly with the proportion of alkali metal, then gradually fall. It was thus observed that, when mercury is heated under paraffin to 250°, and then some sodium is added in portions, the whole mass solidifies with four to five per cent. of sodium; but with further addition of some percentages the mass fuses completely.

The results of their investigation are summed up by the authors as follows:—A survey of the results described shows, for a series of amalgams, that even with moderate heating they do not furnish determinate compounds.

The amalgams of gold, silver, copper, bismuth, lead, tin, zinc, and cadmium lose their mercury entirely, or nearly so even at or under the boiling temperature of mercury. Where no mercury remained, the cause is to be sought rather in a mechanical exclusion than in a chemical action. But, on the other hand, the easy decomposability of these amalgams evidently offers no proof that there are no chemical compounds in them.

For the rest, if we consider the great variability of amalgams, together with the fact that, in squeezing the so-called mercury solutions of metals, these latter do not remain behind, but certain mercury compounds, the view acquires the greatest probability, that at least very many amalgams may be, indeed, molecular combinations, but in fixed relations.

Most pronounced does chemism appear to be in the amalgams of potassium and sodium. They lose their mercury extremely slowly, even at the boiling point of sulphur, as also in a gas current, and so in circumstances highly favourable to removal of mere mixed substances. The remarkable relations, too, as regards the melting point, seem to speak for the presence of true chemical compounds. Probably these amalgams, at a comparatively low, as well as at a high temperature, consist of different compounds, none of which, however, have a durable existence, and therefore recurrent, fixed relations of composition are not to be met with. Alkali-metal amalgams of fixed composition would probably be obtained on production of larger quantities of amalgam; perhaps also by heating considerably above the boiling temperature of mercury.—*Scientific American.*

Reasoning Power in an Indian Crow.—Some years ago I sat with my partner in the verandah of our office at tiffin. Our regular guest, a broken-legged crow, sat on the window sill. In the bread basket was a piece of crust dried by the land wind as hard as a brick bat. I threw it to the crow who picked at it many times, but found it much too hard for his beak. He then evidently sat himself to find a remedy for so sad a state of things. He looked at it with his head on one side, then he took another view of it from the other side, and seemed fairly puzzled. All at once a bright thought struck him, he seized the crust, flew with it to the washhand basin in the corner, shook the morsel well for a minute in the water, flew back to his old place on the sill and gobled up the now soft crust triumphantly. If that did not betoken reason I do not know what could.—Kah Kah.

Sussex Archæological Society.—An interesting addition has just been made to the already large collection of antiquities in the possession of the society, deposited in Lewes Castle. It consists of a cinerary urn, probably of the British-Romano period, about nine inches in height. The vessel is of sun-dried clay, and about seven inches in diameter at its widest part, the mouth being about five inches. It was discovered by some labourers engaged in flint digging on Mr. Homewood's farm, at Jevington, a few days ago. They were working at the foot of Jevington-hill, and came upon several urns embedded in a quantity of loose flints, lying about two feet below the surface of the down land. There were no tumuli or other outward indications that the spot had been used as a burying-place. Unfortunately, the greater part of these relics were destroyed by the picks of the labourers before the nature of the discovery became apparent. One, however, remained intact, and this fact was communicated to the hon. secs. of the society, who at once organised an expedition to the spot. The visit was made on Wednesday, and the "find" carried off in triumph. Those present were the Rev. W. Powell, the Rev. P. de Putron, Mr. R. Crosskey Mr. J. C. Lucas, and Mr. Griffith.

XUM

SILVERED-GLASS TELESCOPES.

HAVING had the first silvered-glass telescope made by Mr. Browning with a With mirror, my experience is longer than that of most others in this country. The instrument was used for many years on the north side of London, and for the last thirteen, or so, in the country. I find the silvering lasts well for five or six years, and then about one-fifth of the light is lost by portions that have come off in the rare, but occasional, rubbings with a dry soft leather. The silvering must on no account be touched unless it is perfectly dry. An appearance of tarnish will usually vanish by exposure to the sun on a warm day. I never found real tarnish, oxydation, or sulphurisation occur to give trouble, even in London. The mouth of the telescope-tube is covered when the instrument is not in use, but no cover has been put over the mirror itself. As compared with a refractor of the same size, the dividing power is equal in good weather; the light, of course, is less. My first observatory was composed of glazed sashes; the second of canvass, stretched on a wooden frame, strengthened with T iron. In both, the internal temperature differs little from the external when the doors are open. This is of great importance. I have known several vexatious failures of good instruments in observatories with thick walls. Reflectors are more easily affected in this way than refractors. The Newtonian pattern, and moderate length of tube, enables nearly all observations to be made in convenient attitudes, which is far from being the case with refractors. The observer should have a stool to stand upon, so as to bring the eye to the level of the eye-piece when the telescope points to the zenith; and another to sit upon, about as high as a chair, for a 6-in. or 8-in. instrument, with the top on a sliding tube, that can be fixed at different heights by a cross-piece. The construction is: one square tube inside another; a series of slots on two opposite sides of the inner one, and one slot on each corresponding side of the outer one, for the cross-pice to run through. HENRY J. SLACK.

HISTORY OF INEBRIETY AS A DISEASE.—Under this title, a communication appears in the *Detroit Medical Review*, from the pen of Dr. Crothers, of Hartford, Conn. He points out that inebriety was recognised as a disease, long before insanity was thought to be other than spiritual madness—or a possession of the devil. This disease was hinted at in an early age of the world, and is by no means a modern idea. On an old papyrus found in one of the tombs of Egypt, dating back to a very ancient period, was a very significant passage referring to an inebriate who had failed to keep sober. Many of the sculptures of Thebes and Egypt exhibit inebriates in the act of receiving physical treatment from their slaves, such as purgatives, rubbings, or applications to the head and spine. Herodotus, five centuries before the Christian era, wrote "that drunkenness showed that both the body and soul were sick." Diodorus and Plutarch assert "that drink madness is an affection of the body which hath destroyed many kings and noble people." Many of the Greek philosophers recognised the physical character of inebriety, and the hereditary influence or tendencies which were transmitted to the next generation. Laws were enacted forbidding women to use wine, and young boys were restricted. In the first century of the Christian era, St. John Chrysostom urged that inebriety was a disease like dyspepsia, and illustrated his meaning by many quaint reasonings. This was the first clear distinctive recognisation of the disease which had been hinted at long before. In the next century Appian, the Roman jurist, referred to the irresponsible character of inebriates, and the necessity of treating them as sick men. Many of the early and later writers of Roman civilisation contain references to drunkenness as a bodily disorder, not controllable beyond a certain point, which resulted in veritable madness. Little reference was made to this theory until the thirteenth century, when one of the Kings of Spain enacted laws fully recognising inebriety as a disease lessening the punishment of crime committed when under the influence of spirits. In the sixteenth century the penal codes of France and many of the German States contained enactments which recognised the disease character of inebriety. All punishment for crime committed during this state varied according to the condition of the prisoner at the time. In 1747 Condillac, a French philosopher, wrote expressing clear views of the disease of inebriety, also that the State should recognise and provide means for its treatment. He asserted that the impulse to drink was, like insanity, an affection of the brain which could not be reached by law or religion. Dr. Benjamin Rush, of Philadelphia, in 1790, set forth the same theory, supported by a long train of reasoning. To him belongs the honour of first elaborating this subject and outlining what has been accepted half a centur after.—*Medical Press and Circular*

PREHISTORIC RESEARCH IN RUSSIA.

AT the meeting of the Imperial Geographical Society of St. Petersburg, Nov. 16, 1881, an account was given of the expedition of M. Malakhof, whose anthropological investigations, which were terminated in October last, had for their principal object the examination of the caves of prehistoric man discovered by him in 1880, and the search for traces of primitive cultivation in the Southern Oural, a region previously unexplored from this point of view. In his journey he examined the districts of Urjum and Velabuga, in the government of Viatka, a large portion of that of Perm, and the Troitsk district in Orenburg. In the first-named district he discovered, near the village of Verobieva, prehistoric remains, including a number of objects in metal and bone, and fragments of clay vases. The bones were those of the deer, bear, wild boar, beaver, horse, &c. In the Governments of Peru and Orenburg, M. Malakhof explored a number of caverns without success, but he was more fortunate in his researches on the shores of the Aiatsky, Shiguirchi, and Vurinsky lakes, and the rivers Miass and Isseta where he made a large collection of arms of polished silex and objects in bone, clay, bronze, and iron. These results lead to the conclusion that in the Oural region, the passage from the stone to the metal age took place in a wholly independent manner owing to the abundance of the metals. In one tumulus he found a skeleton with heavy copper ornaments, and in other excavations he met with a considerable number of bronze idols, amulets, and articles of gold. M. Malakhof has also copied the red hieroglyphic inscriptions found on rocks along various rivers. This find of pure copper articles in Siberia, in addition to those recently in Switzerland, renders it probable that there may have been a pure "copper age" before that of bronze, as thought by some anthropologists.

A MEMBER OF THE SOCIETY OF BIBLIOLOGICAL ARCHÆOLOGY.

SPEECH AMONGST FOWLS.—As Lieutenant-Colonel Souzel and some correspondents still keep the subject of animals and their doings to the fore, I send the following:—Some years ago I saw two young cockerels fighting in an out-of-the-way corner of a large poultry-yard. A hen, probably the anxious mother of one of the young heroes, came up, and, by voice and action, did her best to put an end to the encounter, but without success. She then went away, and presently returned, bringing with her a fine cock. The new-comer made straight for the combatants, who were again hard at it, administered a few vigorous pecks, and walked off with his loving spouse, leaving the youngsters very crestfallen and as peaceable as quakers. The cock's appearance on the scene was not accidental, the hen having gone to the other side of the yard, about thirty yards off, to find him, and having come back beside him, almost arm-in-arm with him [!] This, coupled with the fact that, on his arrival, he seemed to know exactly what to do, seems to point to some fairly well-developed means of communication between fowls, though, unfortunately, I either could not see or did not notice, what actually took place at the meeting. The incident itself is ludicrously human, and is, in fact, an Æsop's fable in real life for little boys just outgrowing the nursery.—ROSEMONT GEARY.

INTELLIGENCE IN SWANS.—The following extract from Yarrell's History of Birds may interest your readers. Mr. Yarrell writes:—"I am indebted to the kindness of Lord Braybrooke for the following account of a female swan on the river at Bishop's Stortford. This swan (*Cygnus olor*) was eighteen or nineteen years old, had brought up many broods, and was highly valued by the neighbours. She exhibited, some eight or nine years past, one of the most remarkable instances of the powers of instinct that was ever recorded. She was sitting on four or five eggs, and was observed to be very busy in collecting weeds, grasses, &c., to raise her nest; a farming-man was ordered to take down half a load of haulm, with which she most industriously raised her nest and the eggs 2½ ft.; that very night there came down a tremendous fall of rain, which flooded all the malt-shops and did great damage. *Man* made no preparation, the *bird* did. Instinct prevailed over reason. Her eggs were above, and only just above, the water."—(Brit. Birds. III., p. 207, 208. Second Edition.) Two phenomena present themselves here. (1) The prescience in the bird of impending rain; (2) the means adopted by the bird to provide for the security of her nest and eggs. The first question may be, perhaps, mainly independent of any direct conscious mental emotion, and may be due, as suggested by Yarrell, to the susceptibility of the feathery covering of the bird to atmospheric electrical changes, and what we call *instinct* may have foretold impending rain; but I do not see how we can interpret the swan's act in providing for the safety of her nest, otherwise than by attributing it to direct *reasoning* power.—W. HOUGHTON.

XUM

MORNING WORK.

PERHAPS, on the whole, moderately early rising is now a commoner practice in cities than it was forty years ago. It seems strange that the habit of lying in bed hours after the sun is up should ever have obtained a hold on the multitude of brain-workers, as undoubtedly it had in times past. Hour for hour, the intellectual work done in the early morning, when the atmosphere is as yet unpoisoned by the breath of myriads of actively-moving creatures, must be, and, as a matter of experience, is incomparably better than that done at night. The habit of writing and reading late in the day and far into the night, "for the sake of quiet," is one of the most mischievous to which a man of mind can addict himself. When the body is jaded, the spirit may seem to be at rest, and not so easily distracted by the surroundings which we think less obtrusive than in the day; but this *seeming* is a snare. When the body is weary, the brain, which is an integral part of the body, and the mind, which is simply brain function, are weary too. If we persist in working one part of the system because some other part is too tired to trouble us, that cannot be wise management of self. The feeling of tranquility which comes over the busy and active man about 10·30 or 11 o'clock ought not to be regarded as an incentive to work. It is, in fact, the effect of a lowering of vitality consequent on the exhaustion of the physical sense. Nature wants and calls for physiological rest. Instead of complying with her reasonable demand, the night-worker hails the "feeling" of mental quiescence, mistakes it for clearness and acuteness, and whips the jaded organism with the will until it goes on working. What is the result? Immediately, the accomplishment of a task fairly well, but not half so well as if it had been performed with the vigour of a refreshed brain working in health from proper sleep. Remotely, or later on, comes the penalty to be paid for unnatural exertion—that is, energy wrung from exhausted or weary nerve centres under pressure. This penalty takes the form of "nervousness," perhaps sleeplessness, almost certainly some loss or depreciation of function in one or more of the great organs concerned in nutrition. To relieve these maladies—springing from this unsuspected cause—the brain-worker very likely has recourse to the use of stimulants, possibly alcoholic, or it may be simply tea or coffee. The sequel need not be followed. Night work during student life and in after years is the fruitful cause of much unexplained, though by no means inexplicable, suffering, for which it is difficult, if not impossible, to find a remedy. Surely morning is *the* time for work, when the whole body is rested, the brain relieved from its tension, and mind power at its best.—*Lancet*.

A PANTHER IN VERMONT.

"F. H. H.," in the *Scientific American*, writes as follows:—It seems to me not improper that some mention should be made in your columns of the remarkable specimen of puma (*Felis concolor*, L.) which was recently killed in the town of Barnard, Vermont. We are not surprised at the stories related by our forefathers of hunting wolves, bears, panthers, and other large animals on spots long since thickly settled by man, nor at the strange experiences of the woodsman when his axe was first heard to ring in the primeval forest. It is not an uncommon thing, indeed, now, for such animals as deer, catamounts, or bears to be shot or trapped in many towns on the northern border of New England; but when a full-grown puma, one of the most savage of wild animals on our continent, is taken prowling about the outskirts of a town, in a State which is settled to such an extent as Vermont, we are enabled to realise the condition of the wilderness as it once was, and the nature of those animals with which it was denizened.

The circumstances of this remarkable hunt are as follows:—

Some boys, who lived in Barnard, went out after partridges on Thanksgiving Day, Nov. 24, 1881. They soon discovered the fresh tracks of some large animal, and on following a short distance crossed their own path. Being frightened at this circumstance, and also from catching a glimpse of the animal, they hastened back to the house of a neighbour, who soon accompanied them, armed with a shot-gun, together with his son, who carried a rifle. They presently sighted their game, which they chased to a thicket, where it was dislodged several times, but finally shot. On dragging the animal out, what was at first thought to be a bear proved to be a female panther of the largest size, measuring 7½ feet from tip to tip, and weighing nearly 200 pounds. It would seem strange at first that the animal was not more savage, that he did not charge his pursuers, and kill them at once. This may, however, be partially accounted for from the fact, as afterwards appeared, that it had made its supper on two sheep in Pomfret only the night before."

This is the second or third of the species killed in the State since the beginning of the century, and in all probability it will be the last. The animal was in fine condition, being in its new fur, and showing no signs of having been previously trapped or wounded. The upper right canine was truncated at about the middle, but this might have been done in a skirmish when the puma was young. In general, the colour of the upper parts was tawny-yellow, with a darker wash of the same along the dorsal line, on the tip of the tail, the ears, and face. The whole animal presents in a striking and exaggerated manner the form and features of the ordinary domestic cat. The tail is straight and larger in diameter at the base, the neck short, the ears erect and pocketed. The dentition is precisely similar, the canines being conical, and rising an inch or more from the jaws. The paws are 7 in. wide when the fingers are spread, and conceal a very formidable set of claws.

This panther is supposed to have made the town and vicinity where it was taken, its home for seven or eight years, and on several occasions has been seen or heard from. One hundred and thirty sheep have probably fallen victims to its rapacious maw, as the town records would indicate.

JELLY FISH.

A YEAR ago the Duke of Argyll made a famous *mot*, comparing the Ministry to jelly-fish, "who fancy they swim, while they only float." As the learned author of the "Reign of Law" has made so great an error, I fancy it must be general. Jelly-fish (*margouilles*) cannot, indeed, dart about like fish proper, but they can swim, I should say, about two miles an hour. Their slowness is compensated by a faculty which enables them to know, if near a lee shore, that they must swim against the wind—otherwise they will be stranded, and die. But this faculty does not serve them so far as to announce an *opposite* shore. Thus, after a N.E. gale here, the shore is strewn with hundreds of jelly-fish, often 2 ft. in diameter. These were all, when the gale began, off the coast of Noismontier (to S.W.) ten miles away. They began to swim away from that lee shore, and after several hours' intelligent exertion, are lost at last (poor finite things!) on the *windward* coast of the bay. The fishermen say that a gale here kills, in this way, nearly all the *margouilles*, on one coast or the other, and that for a long time hardly any are in the sea at all; whereas on an open coast they almost all effect their escape. They are not afraid of shallow water, if calm; I have often watched them, and touched them with hand or foot in bathing. They are slightly electric, leaving a sting or tingling on the skin.

It is said the glow-worm is always female, *i.e.*, the (winged) male has no light. This is not so. I have seen them crawling on the floor in the dark—a spot of light like the point of a match—and on bringing a light have found a male glow-worm. Some worms—centipedes—emit a most brilliant light when crushed.

HALLYARDS.

INTELLIGENCE IN BIRDS.

I HAVE not said anything about the crows, which are a feature of Yezo, and one which the colonists would willingly dispense with. There are millions of them, and in many places they break the silence of the silent land with a Babel of noisy discords. They are everywhere, and have attained a degree of most unpardonable impertinence, mingled with a cunning and sagacity which almost put them on a level with man in some circumstances. Five of them were so impudent as to alight on two of my horses, and so be ferried across the Yurapugawa. In the inn-garden at Mori I saw a dog eating a piece of carrion in the presence of several of these covetous birds. They evidently said a good deal to each other on the subject, and now and then one or two of them tried to pull the meat away from him, which he resented. At last a big strong crow succeeded in tearing off a piece, with which he returned to the pine where the others were congregated, and after much earnest speech they all surrounded the dog, and the leading bird dexterously dropped the small piece of meat within reach of his mouth, when he immediately snapped at it, letting go the big piece unwisely for a second, on which two of the crows flew away with it to the pine, and with much fluttering and hilarity they all ate, or rather gorged it, the deceived dog looking vacant and bewildered for a moment, after which he sat under the tree and barked at them inanely. A gentleman told me that he saw a dog holding a piece of meat in like manner in the presence of three crows, which also vainly tried to tear it from him, and after a consultation they separated, two going as near as they dared to the meat, while the third gave the tail a bite sharp enough to make dog turn round with a squeal, on which the other villains seized the meat, and the three fed triumphantly upon it on the top of a wall. In man places they are so aggressive as to destro

crops, unless these are protected by netting. They assemble on the sore backs of horses and pick them into holes, and are mischievous in many ways. They are very late in going to roost, and are early astir in the morning, and are so bold that they often came "with many a stately flirt and flutter" into the verandah where I was sitting. I never watched an assemblage of them for any length of time without being convinced that there was a Nestor among them to lead their movements. Along the sea-shore they are very amusing, for they "take the air" in the evening, seated on sand-banks facing the wind, with their mouths open. They are threatening to devour the settlers, and a crusade is just now being waged against them, but their name is Legion. ("Unbeaten Tracks in Japan," vol. ii., p. 149.)—From *Nature*.

FAIRY "FOLK-LORE" OF SHETLAND.

LIKE all of the class to which the majority of them belong, the Shetlanders are strongly imbued with a belief in the supernatural. They seem half ashamed to confess it, but the fact is soon discovered by those who have any intercourse with them. Many of their superstitions are of Scandinavian origin, and have been handed down from their Norse ancestors.

The principal character in all their goblin tales is the drow, or fairy, a being supposed to have great power and malignity, and capable of doing great harm both to men and cattle. The drows are believed to inhabit knolls or hillocks, and when a person meets them, if he has not a Bible in his pocket, he must draw a circle round about him and say, "In God's name, keep back." In paralysis the drows are said to have taken away the affected limb, and to have put a log of wood in its place, the entire absence of sensation in the diseased member being adduced as proof of the assertion. Consumption is said to be owing to their taking away the heart, and this complaint is thought to be cured by dropping melted lead into water till a triangular piece is obtained, which is hung round the patient's neck, and placed as near as possible to where his heart ought to be.

Within the last few years witches, or wise women, have been consulted in regard to ca-es of sickness, by men apparently possessed of good sound sense, and some of them even elders of the Kirk. The advice given is a sort of charm, similar to those imputed to the witches of the seventeenth century, to which there are many allusions in the various Presbytery records of the period.

Touching for the king's evil is still a devout article of faith in Shetland; but in order to be effectual it must be gone about with a certain degree of ceremony. A silver coin of Charles II., one of a small number expressly manipulated by the merry monarch for the purpose, and bestowed on some favourite from whom they found their way into these islands, is first laid upon the afflicted person's tongue, and then carefully passed over the affected part, in the case of a male, by the third or seventh daughter; in that of a female by the third or seventh son, of a family belonging to one father and mother. In going through this process great precaution must be used lest the person operated upon should obtain a sight of the coin, for then the spell would be broken. We have heard it repeatedly affirmed that those who have been treated in this manner, and according to proper form, have invariably been cured, and great regret expressed that the only remaining coin in the island invested with this healing virtue is lost, and cannot be recovered.

There are also many superstitions connected with the sea. When they are about to set out to the haaf, they think it unlucky to meet a minister, to call anything by its right name, to make a false step, or to do anything else that may be ominous of trouble and disaster. Should any of these accidents occur, they will turn back and refuse to venture, although the weather be propitious, the sea calm, and everything else favourable for the expedition. When at sea the minister is called the "upstander," or the "lodar," the manse is denominated the "lodar's still hod," and the kirk the "banie house;" every part of the boat's furniture in like manner receiving a change of appellation.

The belief in omens and portents is carried in Shetland to a greater extent than one would imagine. Talking to a man one day, we happended to mention the fact of our having seen a lamb which had been dropped very early in the season. To our surprise, he suddenly became very grave, but said nothing. The reason for this change in his demeanour turned out to be an impression that early-dropped lambs were "no canny," and were ominous of evil. "The folk at Hargaetter," he said, after a thoughtful silence, "had a lamb dropped on New Year's Day, and their son was killed by falling over the banks the same year."

The strange reluctance to save a drowning man, from the wild and absurd notion that to do so would be to incur the certainty of receiving some wrong or injury at his hands, and which is so well introduced into Sir Walter Scott's novel of "The Pirate," if it ever was common in Shetland, is now everywhere extinct, and there is not a man in the islands who would hesitate to do all in his power to aid a fellow-creature if he saw him in peril from the waves. It is to be hoped that all such baneful superstitions will soon disappear before the advance of education.

A. W., in "The Antiquarian Magazine."

Diphtheria Communicated by Cats.—Dr. William Bunce, of Oberlin, O., sends a report of the following cases to the *New York Medical Record*, March 4:—On May 1, 1881, he was called to see a boy four years of age, of German parentage, and one of six children; he was found to have diphtheria. On the following day the youngest daughter, two years of age, presented symptoms of the same disease, and on the next day the father and two more children were attacked. After this date all the other members of the family, except the eldest boy, contracted the disease. A thorough examination of the house elicited no source of contagion, but in the barn a cat was found having the characteristic lesions of diphtheria. On inquiry, he ascertained that this cat during its period of sickness had been played with by the children. On August 20, 1881, he saw, with his son, Dr. W. C. Bunce, a lady, eighteen years of age, who had diphtheria of a very severe type, which terminated fatally on the third day. In a short time the disease developed in the mother and remaining two daughters. A half-grown cat in the room was found to have well-marked diphtheritic membrane in the throat; it was also ascertained that its mother and four other kittens had been in the same condition. The girls had endeavoured to cure the cats by removing the deposit, in this way exposing themselves to the contagious influence of the disease. After the recovery of these cases, and the removal of the diseased animals, the spread of the disease ceased. He thinks it fair, therefore, to conclude that the diseased condition of the cats was the cause of the diphtheritic manifestations in the cases reported. Mention is made of these cases as they are of importance in the consideration of comparative medicine.

The Welsh *W*.—We have a fixed and settled conviction that the English language is the very best language in existence, and that all foreign tongues whatsoever are mere lingoes, wholly unworthy of our sublime consideration. Now there is no doubt a great deal of sound truth in this view; for even French and German philologists have been known to hold that English, because of its relative simplicity and logical development, its freedom from the childish fetters of gender and inflection, will ultimately become the common medium of intercourse for the whole world. But our firm and profound belief in the absolute superiority of our own tongue has always made us very disdainful of other people's. There is a genuine substratum of reality in the old joke about that typical John Bull who wouldn't learn French to talk to the mossoos, but thought the mossoos might learn English if they wanted to talk to him. This universal English feeling, however, seems to reach its culminating point when the foreign language with which we have to deal is Welsh. Most "Saxons" have a congenital horror and dread of the Cymric tongue, which they absurdly declare to be full of consonants and absolutely unpronounceable. As a matter of fact, Welsh is far softer and more vocal than our own harsh Teutonic speech, for it lies about half-way between English and Italian, so far as the relative predominance of vowels or consonants is concerned; and lest my reader should view this paradoxical statement with suspicion, taking me for a Welshman in disguise, I hasten to add that I am not in any way connected with Wales, and that I shared all the common Saxon prejudices on this matter myself until I began to learn a smattering of Welsh for philological purposes. Almost all the terror and mystery of those awesome combinations of letters which are wont so greatly to frighten us is removed in a moment, as soon as people have discovered the simple fact that *w* is a vowel, and not a consonant, its phonetic value being merely that of our own *oo*. Cwm and Drws look very terrible indeed until one knows that they are pronounced like Combe and Druce; while the fearsomeness of Llwch disappears entirely as soon as we recognise that it is nothing more than the Scotch Loch in an unfamiliar guise. Yet, in spite of the perfect transparency and regularity of Cymric phonetic spelling, ten thousand English tourists continue every year to talk about those jaw-breaking long Welsh names, which are utterly unpronounceable by English lips merely because they have never taken the trouble to get up the most elementary rules of the language, as they would get up a little German before going up the Rhine, or a little Italian before trying a winter at Rome or Florence.—From "Some English Place Names" in the *Cornhill Magazine* for November.

XUM

Letters to the Editor.

[*The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.*]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

*** *All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters; and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the proper query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

ELECTRIC TELEGRAPH.

[370]—It is pleasing to learn (letter 367, p. 479) that Sömmering's telegraphic apparatus is still in existence, but it must not be forgotten that the idea of the electric transmission of intelligence was not a novel one in Sömmering's day. It is to Charles Marshall, a Scotsman, that we owe the invention of the electric telegraph. He, in 1753, suggested that by erecting a series of wires, one for each letter of the alphabet, and attaching to the end of each wire a ball, near which was suspended a piece of paper free to move towards the ball, and inscribed with a letter of the alphabet, and sending a charge of electricity through first one wire and then another, the pieces of paper at the other ends might be caused to move, and so to indicate the letters of the message which it was desired to transmit. In 1774 Marshall's scheme was realised by Le Sage of Geneva. ALFRED W. SOWARD.

GHOSTS.

[371]—The following details regarding the "War Office Ghost" may be of interest to T. D. In September, 1857. Captain G—— W——, of the 6th Dragoon Guards, left England to join his regiment in India, leaving his wife at Cambridge. On the night between the 14th and 15th November, 1857, she dreamed that she saw her husband looking very ill, and she thereupon awoke, much agitated. When she looked up she saw the same figure standing by her bedside. He appeared in uniform, and seemed to be in great pain. Mrs. W—— at first thought that she must be still asleep, but, by rubbing her eyes, and by listening to the breathing of a child beside her, she became convinced that what she had seen was no dream. In December, 1857, a telegram from the seat of war appeared in the morning papers stating that Captain W—— had been killed before Lucknow on the *fifteenth* of November. This date was further confirmed by the War Office certificate, which was obtained by the family solicitor. Mrs. W——, however, maintained that her husband had died on the *fourteenth*, and not on the *fifteenth* as stated. While the solicitor's mind remained in uncertainty regarding the real date of the death, a curious incident occurred which seemed to confirm Mrs. W——'s opinion. The solicitor mentioned the case to a lady friend, who all her life had had perception of apparitions. She immediately, turning to her husband, said, "That must have been the same apparition that I saw on the evening we were speaking about India." The receipt for an account paid on the same day enabled them to fix on the *fourteenth* as the date. The solicitor was so much impressed by this that he applied to the War Office to find out whether there had not been some mistake about the date. The officials stated that there could be no mistake, as the death was referred to in two despatches from Sir Colin Campbell, and in both the date was given as the *fifteenth*. In March, 1858, a letter arrived from a brother officer, giving an account of Captain W——'s death. This officer, who had been riding beside Captain W—— when he was killed, stated that the death occurred on the *fourteenth* of November. Finally, about a year after the death, the War Office altered the date to the *fourteenth*. JOHN GORDON.

MOON CRATERS.

[372]—*A propos* of the notice at p. 439 of imitations of moon craters, I was greatly struck only a few days since by the very close resemblance to such craters in the impression produced on a smooth surface of sand by a drop of water falling on it from a height of about five or six feet. This is what happened in a greenhouse on the moisture of condensation dropping into a pot sown with seed and thickly covered with sand.

Perhaps, as a small contribution to KNOWLEDGE, this observation may be acceptable. J. POWER HICKS.

COLOURS AT NIGHT.

[373]—Can you tell me what is the explanation of the fact that if a strip of cloth coloured either white, black, or blue be hung up on a pitch-dark night in a place where no lights are visible, and against the sky as a background, it becomes swallowed up, as it were, in the darkness, and is invisible; but that if its colour be red, it stands out as a dark patch on the black sky? I have frequently observed this to be the case, so that I am sure of my fact.

WINTER.

GOLDEN SANDS.

[374]—On the Western coast of India there is a river. To the north of its mouth on the sea beach, during the south-west monsoon, appear many patches of black sand (grains of magnetic iron ore). These patches average five or six square yards area, and are about half-an-inch thick, and lie on the surface of the ordinary yellow sand of the beach. The gold-washing natives carefully scrape off this black sand, during the rains, as fast as it appears, and make heaps of it on the higher beach beyond the reach of the sea at high tides. When the rains are over they wash these heaps for gold, using first a small wooden cradle and then a shallow gun-metal dish, such as is commonly used by natives for their rice, and called a "Kinny." When the black sand is almost all washed away they use quicksilver, make an amalgam, squeeze the excess of mercury out through a piece of washleather, and get rid of the rest by heat, leaving from two to six annas (threepence to eightpence) worth of gold for their day's trouble. My trouble is not how they get the gold from the sand—that is easy enough, I have done it myself a hundred times—but how did the gold find its way down the river and on to the surface of the beach? and why always in company of the black sand?

Not one particle of gold is ever found in the yellow sand. There is no adhesion or attraction between the black sand and the gold, for the latter is as easily washed from the former as from yellow sand when mixed for experiment. I have tried it often. Why did not the gold-dust find its way to the bottom of the river, as it did at once to the bottom of the pan? That is a PUZZLER.

JUPITER IN CASSIOPEIA.

[375]—With reference to your editorial remark respecting my communication on "Jupiter in Cassiopeia" (correspondence column, p. 478), I beg to say the context of the refreshing passage in Schiller's "Wallenstein," naturally suggests the interpretation I put on the words of the poet, and every German reader who knows the force of *dahin* and has, besides, even an elementary knowledge of astronomy only, would at once understand it to mean "yonder." If Schiller had wished to make Wallenstein say, "that he saw Jupiter *in* Cassiopeia," he would certainly have used *darin*, or some similar expression. May I add that few poets have so carefully and accurately worked out the details of their productions as Schiller has done. In his brilliant tragedy of "Wallenstein," he has shown that he had fully mastered the "subject of astrology," and it is quite natural that he should have at the same time occupied himself with the science of astronomy, if he had not done so before.

C. A. BUCHHEIM, PH. D.

XUM

OPEN FIRE-PLACES.

[376].—What are we to do? Fortunately the weather has not been very cold. On page 374 of Knowledge of 3rd inst. our friend W. M. Williams concludes his article on open fire-places, with telling us "to brick up, cement up, or otherwise completely stop up, all our existing fire-holes, and abolish all our existing fires," and then he promises to tell us what to do next. Has he considered how awkwardly we are placed without a spark of fire all this time? Must we unstop the fire-holes? Of course we must wait another week. Must we send our doctor's bill to him, or to you, Mr. Editor?

C. J. Brown.

STARCH IN POTATOES.

[377]—In answer to "Farmer's" last question (329), it may be stated that it has been found that the ratios between the starch and nitrogenous matter in potatoes are not always the same, and that in those potatoes which are the least floury the ratio of nitrogen to starch is the highest, by reason of decrease in starch. The cause seems to rest with the manure, for the same variety, under like conditions of position, soil, and climate, is greatly influenced in composition by the manure applied. E. W. P.

HAIR TURNING WHITE.

[378]—Perhaps the following facts may be of sufficient interest to warrant their insertion in Knowledge. James Trafford, a coach-smith in the employment of Francis Mulliner, coachbuilder, of Liverpool and London, about two-and-a-half years ago burned himself accidently at the root of the middle finger of his right hand. The injury was caused by a bit of red-hot iron flying into his hand. He was obliged to cease work at once, and continued to be incapable for about three months, during which time I watched the consequences of his accident as they made their appearance. He poulticed his hand, and in about a week the swelling broke, upon which matter began to exude. In a week after the breaking of the wound, and while the matter was exuding, his hair began to turn white upon the right side of his head, and a fortnight subsequently he presented the picture of a man with the hair, moustache, and beard, on the right side of his head, snow white, and on the left side jet black. The process continued, the matter was still exuding. Five weeks after his accident, every hair on his head and face was perfectly white. At this time, he was only about forty-two years old. Ten weeks after his accident he was so bald that not a single hair was to be found on his head. A week later his hand healed, and the matter ceased to run. Another week and his hair re-appeared, and grew so fast that in about a month his head and face were again covered with hair. Six months after his accident, his head and face presented their usual appearance—jet black hair, moustache, and beard. I obtained his consent to send this account to another journal, but omitted doing so. It has, therefore, never before been published. H. Smith.

THE LANK YANK.

[379]—Reading on page 457 the remarks of "Canadensis" in this connection, I would add that, having visited New York City in the days of old, besides being collided with many Yanks upon the Australian gold-diggings, it struck me that the leanness of the American is due to the abominable practice of chewing honeydew, accompanied by the constant ejection of the saliva so necessary for the due assimilation, secretion, and digestion of the dietetical delicacies incident to their *Déjeûners à la Fourchette*, &c. Thus was I led to speculate upon the question, "How many of the leaner sort of Americos would suffice to cool the condenser of a 10 H.P. engine?" J. J. A., Liverpool.

THE PERFECT WAY IN DIET.

[380]—May I correct two misprints among the names of advocates of reform in diet cited by me? For Halley read Haller, the well-known German physiologist of last century; and for Daumen read Daumer, the learned Professor of Munich, popularly known as the (for sometime) guardian of Gaspar Hauser, the wild boy of the woods. Also, for Boyle read Bayle, the eulogiser of Gassendi.

It is not pretended that all the authorities quoted by me in your last number, or the very many other admirable writers omitted in my necessarily brief notice, were, in practice, anti-kreophagist. Until some forty years ago there was no kind of Association, no rallying centre for those deeper thinkers, or more humane minds, who, nevertheless, instinctively, as it were, shrank from the cruel barbarism of the slaughter-house. No writer (not actually an abstinent from flesh-meats) has more often protested against the horrifying practice of butchering for food than Voltaire (See, *inter alia*, "Essai Sur les Mœurs," &c., his romance of "La Princesse de Babylone," Dict. Phil., art. "Viands"). It is entirely reasonable to assume that, were he now living, he would be practically, no less than theoretically, a humane dietest. And so of the other prophets of reformed dietetics who lived before the present era of associated reformers. Howard Williams, M.A.

Queries.

[349]—Quotation.—In what literary work shall I find:—"Little things on little wings bear little souls to heaven"?—F.

[350]—Jet.—What is the composition of jet, and how may it have been formed in the rocks? Has it ever been chemically analysed, and with what result?—Whitby.

[351]—Glycol.—Can any reader give the following:—1. Test for glycerine. 2. A practical means of separating glycerine from the residue from palm oil, after the latter has been heated to, say, 320°, and agitated with H_2SO_4?—A. T. Ross.

[352]—Will one of your readers be kind enough to tell me the best method to procure deutoxide of nitrogen from nitric acid?—H. Neird.(?)

Replies to Queries.

[276]—Photography.—For "quality," read "quantity;" and for "prosulphite," read "hyposulphite" of soda.—A. Brothers.

[330]—Climbing Plants.—If W. P. B. will look at Darwin's "Climbing Plants" he will see that the twining, though influenced (*i.e.*, retarded or accelerated), is not caused by the sun. In a list there given of twining plants, fourteen follow the sun (hop), twenty-seven move against the sun (convolvulus). A single revolution is often performed in from six to three hours, or less; not 24.—S. C. Wood.

[336]—In Memoriam.—We have received many replies to this query. Nearly all agree that Longfellow is the poet referred to, naming his poem, "The Ladder of St. Augustine." This reply is given by S. Oliver, K., Clare, S. C. Wood, T. Sulman, J. Stewart, H. J. Fry, Chas. H. Cotton, T. H. M., E. D. G., A. P. Thomas, J. S., H. A. H., T. B. E. C. R. suggests that either Longfellow or St. Augustine was intended (but how could St. Augustine be described as he "who sings to one clear harp in divers tones"?); Gog Magog names Longfellow, but speaks of his poem, "The Balm of Life"; W. C. says Shakespeare, but quotes no passage; Bernerd Batigan also names Shakespeare, *passim*, but especially in the sonnet beginning "Poor soul, the centre of my sinful earth" (in which we can trace very resemblance to the idea referred to by Tennyson); C. Fen says King David; W. G. Finch thinks that reference is made to the Son of David; T. M. Blackie, F.S.A., thinks Arthur Hallam was meant. To myself it seems altogether clear that Tennyson meant a poet living when he wrote, and that that poet was Longfellow.

[369]—Telephone.—From ½oz. to ¾oz. of No. 36 silk-covered wire is required for bobbins. I will try to forward description of transmitter by next week.—G. E. V.

[In answer to G. T. W. M., page 459 of Knowledge, March 24, 1882, there is a book by Dean Alford, "Queen's English," published by Bell & Son, at 5s.—T. G. T.

Replying to query on "Colliery Spoil Banks," p. 477, there is no doubt that the ignition is "spontaneous"—a term rather vaguely applied. All *ordinary* combustion is simply a combination of the combustible substance with the oxygen of the air. Such combustion is going on wherever ordinary vegetable matter is exposed to air and moisture. The leaves that fall from the trees in autumn burn during the winter; so do the dead weeds, grass, &c. In their burning, they raise the temperature of the soil to an extent that is quite measurable when they are abundant. In crossing forests during winter-time, I have on several occasions been nearly ankle-deep in vegetable mud, while the ground outside the wood was frozen hard. This was mainly due to the slow combustion of the fallen leaves. The "spontaneous combustion" of haystacks, of heaps of cotton-waste, and the heat of hotbeds is of similar origin. I have seen a railway siding on fire at Coed Talon, in Flintshire. It was ballasted with cannel-coal slack, which combined with oxygen in this manner. The spoil banks of collieries are made

up of the rock that is taken out in sinking the pits and driving the roads, and most of the rocks—"the coal measures," as the miner calls them—contain vegetable matter, the most characteristic being the "linstay" or "linsey" rocks, so named by the collier on account of their resemblance to the striped "linstay" or "linsey" of which his wife makes her petticoats. This rock consists of alternate bands of black vegetable matter and of sandstone. The friable shales of the coal measures are similarly interstratified. It is this vegetable matter (in some cases increased by coal screenings) that fires on the pit bank. The tendency to such combustion is increased by the presence of iron pyrites—a compound of iron and sulphur, both of which are combustibles and do burn, or oxidise, readily when exposed to air and moisture. Iron pyrites is especially liable to such spontaneous combustion, and is thereby converted from its gold-like insoluble form into the readily-soluble "green vitriol," or sulphate of iron. This explains the sulphurous fumes to which "One who wants knowledge" alludes.—W. Mattieu Williams.

Answers to Correspondents.

*** *All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule* 3, *free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

Lady Stewart. Regret very much that an advertiser in our columns has been so dilatory (I trust that is the limit of his offence). Your letter has been forwarded to the publishers, and due measures will be taken.—Gladiator. Fencing hardly a scientific subject. As it chances to be a favourite exercise of mine, I will give my own ideas about your difficulty. You ought, considering the name you adopt, to be able readily to meet the *un, deux,* and *un, deux, trois,* which your opponent is so fond of using, in the ordinary way. If I were you, I would not give in till I had acquired the knack. But, if you cannot, try what Mrs. Glasse would call "another way." Follow him with the round parade. This will bring him up in carte, instead of carte over the arm, and all he can do is to try carte over the arm again, which you can treat in the same way, following up his blade. Remember, though, that this parade is not in itself a defence. A good fencer will corkscrew in upon you, if you forget this. The parade simply changes the direction of your guard. Thus if you meet a thrust in carte over the arm with the round parade, you are engaged in carte and must defend accordingly, that is towards the left, not towards the right, as in the simple parade for that thrust. So if you oppose the parade to *un, deux,* &c., you are safe enough during your opponent's extension, but as the lunge is delivered the parade merges into defence in carte. The advantage of the round parade is that you know exactly where your opponent's blade will be when the thrust is delivered.—Priam. You may be surprised to learn that many of the marks of parenthesis you counted so carefully were added *after the papers were in type,* because, on careful consideration, they seemed more suitable than commas. Macaulay (whose name you misspell Macauly) used to boast that he seldom used the semicolon (not the parenthesis.) Whately, who severely rebuked the improper use of the parenthesis, used it more freely than most writers. In fact, every logical writer must use them, to write clearly and correctly. If it were not in very bad taste to comment on faults of style, I would point out that your letter would be all the better (at any rate, much more easily understood) if you had used the parenthesis more freely. Take, for instance, this sentence:—"It has been on my mind ever since the first number of Knowledge to call attention (in a friendly way) to a small evil, on the part of the editor himself, and (in degree) of some contributors, of the constant use of the parenthesis in the articles." Here, I have added parentheses where they are obviously not merely necessary, but essential to the grammatical accuracy of the sentence. Without them, you might mean "a friendly way to a small evil," and "in degree of some contributors." Your first sentence would further be improved by the addition of a dash after "evil" and "contributors," though, even then, "a small evil of the constant use" would be hopelessly ungrammatical. In fine, my dear sir, every sentence of your letter suggests the repetition in your case of a small quotation I have already had to use in reference to correspondents who, having evidently had small experience themselves, undertake to teach me how to do what has been the business of my life. "Teach not a parent's parent," I would beg of you, "to extract, the embryo juices of an egg by suction;" the good old lady can the feat enact quite irrespective of your kind instruction.—Arthur Buchheim. Do you make no distinction between the ordinary use of the term logarithm and its use in the theory of functions? Or, supposing you do draw such a distinction, do you suppose our querist referred to the latter use of the term? What you so carefully explain to me is as familiar as the interpretation of imaginary expressions (of the imaginary cube roots of unity, for instance). Of course, log $(-a)$ has its meaning, just as $\sqrt{-a}$ has; but equally of course that meaning is outside the ordinary use of the function, precisely as $\sqrt{-a}$ means something which cannot be defined in ordinary arithmetical or algebraical terms. —R. W. R. Yes, that is a fair enough account of apparent planetary motions.—J. Smith. Have never heard that telescopic work injured the eyes appreciably. Galileo was certainly not blinded by ordinary telescopic observation. Milton died blind, but that does not prove that making poetry hurts the eyes.—William G. C. Yes, "the subject has taken up space in Knowledge,"—scarcely a reason for continuing the discussion.—H. Judge, T. G., E. C. R., A Constant Reader of "Knowledge," J. Harvey, Spoopendyke, and others. Queries vague, trivial, or for other reasons unsuitable. In future, questions relating to books on special subjects cannot be admitted, though regular contributors on such subjects may indicate books they consider suitable. Advantage has been taken of our Query column to ask questions and to answer them in such a way as to advertise books.—Oudeis. The query escaped our attention or it would not have been inserted. Of course, "Clubs are trumps" is the only correct expression.—M. Soc. Bib. Arch. Third part shall appear very soon.—Jos. Gainswood. The appearance in question has no scientific interpretation, though several well-meaning (but rather foolish) attempts have been made to find one. The story seems to have had its origin in astrological fancies.—Excelsior. Thanks. Corrections made.—Cambrian. Yes; all satellites were probably formed that way, and their greater or less distances would indicate greater or less antiquity.—Clare. Most readers know about Dean Alford's book on the Queen's English. The other matter is very aptly described by you as "no business" of a certain correspondent's.—A. C. Day. Those combination systems have always failed in the working.—Hint. Very likely I may. "I do not say" I won't; "but *Time,* my Christian friend."—Alice Bodington. Thanks, but you will have noticed that Mr. Clodd is attending to Palæolithic Man.

BIOLOGY.

H. St. Maynard asks for the name of any good work on "Ants." He should consult Sir John Lubbock's "Scientific Lectures" (Macmillan), and also the "Transactions of the Linnæan Society," for much original information respecting the habits of these insects. The price of Lubbock's "Scientific Lectures" will be ascertained on application to the publishers, Bedford-street, Covent-garden.—A. Pontwood asks how Huxley divides the two orders of birds (*Ratitæ* and *Carinatæ*). This is a sample of a question, the answer to which would simply represent a chapter of the most technical details of comparative anatomy. I advise "A. Pontwood" to consult Huxley's "Vertebrata;" but I may add that the *Ratitæ* (ostriches, &c.) are classified by the structure of the wing and by the nature of the haunch-bones. The *Carinate* birds are divided into sub-orders, primarily by the nature of the vomer-bone of the skull, and by its relations to other bones (*e.g.,* the maxillo-palatines). Thus, the *Dromæognathæ* (Tinamous) have the vomer, &c., resembling the emus. The *Schizognathæ* have "the lateral maxillo-palatines ununited in the middle line, separated from the vomer by a fissure, and with the vomer pointed in front" —and so on. Unless "A. Pontwood" is prepared to enter upon a study of comparative anatomy, he need not attempt to understand the details concerning which he writes.—A. J. C. W. —Diet of Tortoises. Vegetable matters, especially lettuces and the like; a little milk occasionally.—Aquarium. See Mr. Gosse's book on the "Aquarium" (Van Voorst).—Newts. These amphibia should be kept (if water newts) in clear water, with growing plant-life, and with a dry resting-place admitting of their leaving the water occasionally. Food, worms chiefly. They are common in ponds in the country, or may be found beneath stones near water. See Bell's "British Reptiles."—J. Hamson. 1. The human skeleton you speak of found in the cave at Mentone, and spoken of by Rivière (Paris, 1873), was found in March, 1872. The cave is called La Barma du Cavillon. No metal nor any polished flint was found associated with it. The skull was dolichocephalic; and the whole surroundings (as well as the teeth) indicated a savage life. The skeleton is of *Palæolithic* age

The thigh-bones were very strongly ridged, indicating great muscular power; and there were also found associated with it the cave bear, tiger, hyæna, and woolly rhinoceros. 2. The Engis skull was found near Liége. Dr. Schmerling figured it in his work, but whilst observing that it was too imperfect to afford any determination of facial angle, he assumed that it belonged to an individual of low intellectual development. Mr. Bush says it might be matched by individuals of European race, and in this opinion most geologists and anatomists agree. This skull is of Pleistocene age. Huxley says that "the capacity of the interior of this fragmentary skull has not been ascertained ("Man's Place in Nature," page 128). Both Neanderthal and Engis skulls belong to the Pleistocene period. The former is certainly the lowest type of human skull known. 3. Mr. Fiske's "postulate" (KNOWLEDGE, page 408, No. 11) simply recapitulates an underlying assumption of the whole statement of the case for evolution. It is a needful postulate for the person who is ignorant of, or opposed to, evolution. For one like Mr. Hamson, who admits the truth of evolution, it is simply an additional proposition.

ELECTRICAL.

WM. FLEMING. Good examination certificates have some little weight with electrical engineering firms when selecting their *employés*, but the industry is very young, and practical knowledge *is*, therefore, generally looked for. Try the examinations of the City and Guilds Technical Institute, Gresham College, E.C. The secretary will send particulars on speedy application. You may study with advantage Sprague's "Electricity," Culley's "Telegraphy" (the latest edition—seventh), and Urquhart's "Electric Light."—G. H. HARRIS. It is not so unusual as you imagine to see a magnetic needle in which both ends are of the same polarity. If you carry your experiment a little further, you will find the other or opposite pole in the middle of the needle, there being in this case two neutral zones. The centre pole will, of course, be equal in strength to the sum of the two end poles.—JAMES GRUNDY. 1. The manganic di-oxide and carbon in the Leclanché cell should be about equal in quantity. Covering in the porous pot with pitch or marine glue keeps the contents in position, but be careful to leave a vent-hole for the escape of any gases that may be evolved in the cell. Use crystallised sal ammoniac and water for the outer cell. 2. You can only determine the value of the current obtained by measurement Have you the means of doing this? 3. What is the specimen of wire you enclosed? Is it aluminium? It is too dirty-looking for platinum, nor does it test like it. The resistance of about one foot of it is only 0·006 of an Ohm. One hundred Leclanché cells failed to raise a couple of inches to redness, but 140 cells make 3 feet red hot. 4. The electro-motive force of a Leclanché cell is about 1·6 Volts, the Grove being 1·8.—W. G. FINCH. Electrical tricycles are not the only things missing in the Crystal Palace Exhibition. To work a tricycle you would require a motor, with a battery, or its equivalent, to work it. I know of none before the public, but believe one is soon to be offered by a Manchester house.—A. E. SMITH. A series of articles on dynamo-electric machines is now appearing in this journal. I am not quite sure what you require. If you do not see it, write again.—S. B. Galvanic batteries of the Daniell, Leclanché, and bichromate types are used by the Postal Telegraph Department. The electric light *could* be produced by them, but neither efficiently nor economically. £5 or £6 spent in Bunsen or Grove cells would yield an electric light for three hours or so.—H. A. B. "Lightning protector" is a better name than "lightning conductor." Its chief work is to *neutralise* the electricity of the overhanging cloud by discharging into it an equal quantity of the opposite kind, which is drawn or attracted from the earth by the electricity in the cloud. Should it happen that the protector cannot discharge quickly enough, then the cloud-charge strikes towards the earth; and as the protector is the nearest object, and offers the easiest means of passage, the electricity from the cloud passes through the protector to the earth. Consequently, the more *good* protectors you have, the safer will your house be, as each one added is another possible channel of escape.—J. R. WILSON. Read preceding answer. The proper height of the lightning protector will depend on the area it is intended to protect. Copper, in the band or rope form, with platinum points, is the best material. As, however, there is considerable diversity of opinion on the question of measurement, &c., the subject will be dealt with in an article in this journal a week or two hence, when it is hoped you will find all you require.—C. H. W. Dynamo-electric machines are of so recent a birth that it is hardly fair to look just yet for any great flood of literature upon the subject. Efforts will, however, be made to supply shortly the information required by those who are anxious to make machines for themselves.

Our Mathematical Column.

PROBABILITIES.

To the two examples in our last the following may be added:—

3. *How many trials with a full pack give an even chance of cutting any given card (of a given suit)?*

The chance of failing to cut the given card in a single trial is $\frac{51}{52}$, in two trials $\left(\frac{51}{52}\right)^2$, and so on. If n be the number of trials, which will give an even chance, we have—

$$\left(\frac{51}{52}\right)^n = \frac{1}{2}, \text{ or } \left(\frac{52}{51}\right)^n = 2.$$

Hence, taking logarithms—

$$n\ (\log. 52 - \log. 51) = \log. 2,$$

$$\text{or } 84331n = 3010300$$

$$\text{whence } n = \frac{3010300}{84331} = 35{\cdot}7$$

whence 36 trials will give slightly more than an even chance of drawing the named card.

4. *In how many trials will there be an even chance that dealer at whist will have all the trumps.*

The number of possible hands which dealer can have is the number of combinations of 12 cards which can be made out of 51, and this number it will be found on trial is 158,753,390,900. Hence the chance that he will *not* have all the trumps.

$$= \frac{158,753,389,899}{158,753,389,900}$$

and to give the number of trials, n, in which there will be an even chance that dealer will have all the trumps, we have the equation

$$\left(\frac{158,753,389,899}{158,753,389,900}\right)^n = \tfrac{1}{2}$$

$$\text{Whence, approximately, } \left(\frac{15,875}{15,876}\right)^{\frac{n}{10,000,000}} = \tfrac{1}{2}$$

And taking logarithms, $\frac{n}{10,000,000}(\log. 15,876 - \log. 15,875) = \log. 2$

$$\text{or } 273,555n = 30,103,000,000,000,000$$

[To get the difference 273,555 corresponding to logarithms to ten places of decimals subtract logarithm of 15,860 from logarithm of 15,890, and divide the difference by 30. This gives near approximation to the difference between logarithm of 15,876 and logarithm of 15,865. But to get nearer still, do the same with logarithms of 15,861 and 15,891; add the results thus obtained, which are respectively 27,357 and 27,353, and halve—giving 273,555.]

$$\text{or } n = \frac{30,103,000,000,000,000}{273,555} = 110,400,000,000$$

This, then, in round numbers, would be the required number of trials to give an even chance of a deal in which all the trumps would be in one hand.

PROBLEM 5.—*When the number of possible events, all equally likely, is very great, to determine what ratio the number of trials necessary to give an equal chance of any given event bears to the total number of possible events.*

Let the total number of events be N, then the chance that any given event will not occur in a single trial is $\frac{N-1}{N}$; and if n be the number of events necessary to give an equal chance of the occurrence of that event, we have, as in the previous case,

$$\left(\frac{N}{N-1}\right)^n = 2$$

$$\therefore\ n\left\{\log N - \log.(N-1)\right\} = \log. 2$$

$$\text{or } 2n\mu\left\{\frac{1}{2N-1} + \frac{1}{3(2N-1)^3} + \&c.\right\} = {\cdot}3010300, \text{ where } \mu \text{ is}$$

the *modulus* of the common system of logarithms, or the reciprocal of the Neperian logarithm of 10; whence, when N is very great so that $\frac{2n}{2N-1} = \frac{n}{N}$

$$\frac{n}{N} = \frac{{\cdot}3010300}{\mu} = \frac{3010300}{4342945} = 0{\cdot}6931472\ (= \log_e 2)$$

If we apply this rule to the previous case we get a value very close indeed to that already obtained in another way.

Our Chess Column.

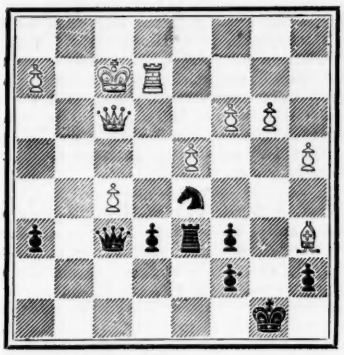
GAME BY CORRESPONDENCE.—(*Continued from p. 485.*)

Position after White's 29th move.

P to B5.

CHIEF EDITOR.

WHITE.



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CHESS EDITOR.

	29. P takes P
30. R to Q5	30. P to B5
31. Q to R3	31. Q to Qsq
32. Q to R 5	32. Kt to B3
33. Q to K2	33. Q to KBsq
34. Q to B3	34. R to Q4
35. K to Bsq	35. R takes R

PROBLEM No. 25.

We think it best to republish this Problem in an amended form.

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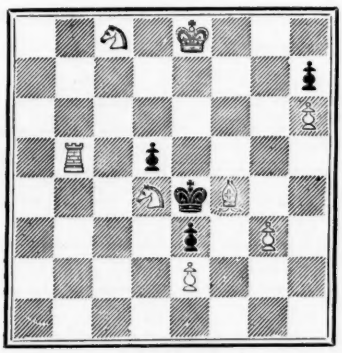

WHITE.

White to play, and mate in three moves.

PROBLEM No. 33.

"Easy and neat."

By C. H. Brockelbank.

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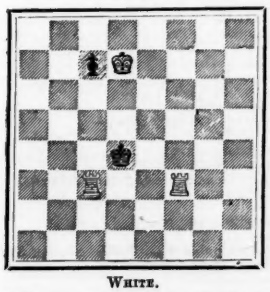

WHITE.

White to play and mate in three.

PROBLEM No. 34.

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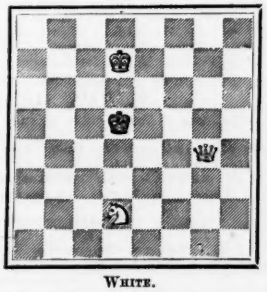


WHITE.

White to play and mate in three.

TWO KNIGHTS' DEFENCE.

White.	Black.	White.	Black.
De Rivière.	Morphy.	De Rivière.	Morphy.
1. P to K4	P to K4	11. Kt to R2	Kt to R2 (*c*)
2. Kt to KB3	Kt to QB3	12. Kt to Q2 (*d*)	P to KB4
3. B to B4	Kt to B3	13. Kt to Kt3	B to Q3
4. Kt to Kt 5	P to Q4	14. Castles	B takes Kt
5. P takes P	Kt to QR4	15. K takes B	P to B5 (*e*)
6. P to Q3 (*a*)	P to KR3	16. Q takes P	Kt to KKt4
7. Kt to K5	P to K5	17. Q to Q4 (*f*)	Kt to KB6 ch (*g*)
8. Q to KB3	Kt takes B	18. P takes Kt	Q to R5
9. P takes Kt	B to QB4	19. R to KRsq	B takes P
10. P to KR3 (*b*)	Castles	20. B to Q2	R to B3 and wins

Notes by Mr. J. Gunsberg in the *Chess Players' Chronicle*.

(*a*) A weak continuation of the Two Knights' Defence, and one which, in our opinion, gives Black the advantage. There are various ways of continuing besides P to Q3, but we are satisfied only with the result of one variation, viz., 6B to Kt5 ch 6P to B3, 7P takes P7P takes P, 8B to K2, as enabling White to retain his advantage.

(*b*) Instead of this unsatisfactory move, it has been suggested by Mr. Proctor to play 10P to QB3; this provides a square of refuge for the Knight on Q4, and also threatens an advance of Pawns on the Queen side; but even this, in our opinion stronger move, does not quite equalise matters, which, through 6P to Q3, rest on a weak foundation.

(*c*) This is against modern "principles," as Mr. Potter would call it. The adopted way of continuing for Black is P to QKt4; he thereby tries to weaken the Queen's side, also relying upon subsequently playing B to QR3.

(*d*) We should have preferred 12B to K3.

(*e*) Taking advantage of the position in a forcible manner; had White not taken the Pawn, his game would, nevertheless, be in a precarious condition.

(*f*) Q to Q3 would have afforded a much better defence.

(*g*) This brings the game to a fine conclusion. White has no defence.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

Dummy.—See answer in No. 20, p. 442.

John Fairweather.—Pawn on 8th is what you please to make it.

G. W.—Thanks for good wishes. Have you begun your games? Solutions correct.

H. A. N.—27, 28, 29—solutions incorrect, see next number.

A. H. Empson *v.* G. Tow.

H. A. N., J. F. Washbrook, J. Griffiths, A. B. Palmer, John Fairweather.—We have corrected and republished this problem this week.

Correspondents whose opponents have not replied to their games can have fresh opponents on application.

Sir,—I see by the "Answers to Correspondents" that there are a good many dissatisfied spirits [unreasonable people!—Ed.] amongst the subscribers to Knowledge. Some want more of one thing, and some more of another, and they all seem to find something that (in their opinion) might be done away with. Now, I, for one, should be sorry to see any of these radical changes, as I think Knowledge is well worth the money, even if taken only for one subject. I am quite satisfied that there are higher-priced papers with a far worse Chess column, and that, I must say, has most attractions for Yours truly, G. W.

Dr. Cheyne states of Sir Isaac Newton, that when he applied himself to the investigation of light and colour, to quicken his faculties and enable him to fix his attention, he confined himself all the time to a small quantity of bread with a little sack and water, without any regulation, except that he took a little whenever he felt his animal spirits flag. . . . . The happy medium which Newton endeavoured to maintain, was just that which would preserve the blood in the fittest state for the purposes of the mind, while intently acting on the brain; and probably not a little of the splendid clearness of his demonstration may be attributed to the success with which he controlled all his bodily propensities, by the moderation which he invariably observed in the management of his stomach.—Extract from "The Use of the Body in Relation to the Mind." 1846. By George Moore, M.D., Member of the Royal College of Physicians.

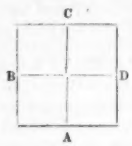
XUM

Our Whist Column.

Sir,—The Chilian method of scoring at Whist may be interesting to those readers of Knowledge who are unacquainted with it. It seems to be preferable to our own.

No honours are counted, and the score is kept in the same way as at Short Whist, with the addition of points for the tricks which one side make more than the other during the rubber. By the usual method, one side may win, even without the assistance of honours, having made fewer tricks than the losers, and good cards beyond those required for the necessary number of tricks are thrown away. The marking is easily done by each player keeping a score—one, on each side, the usual score; the other the + and − account: *e.g.*, let A, B, C, D, be players,—

1st Game.—1st hand. AC win 4 by tricks.
2nd ,, BD ,, 3 ,,
3rd ,, AC ,, 5 ,,



	Score at Short Whist.	Chili Score.
1st Game.—1st hand	A scores 4	C scores 4.
2nd ,,	B ,, 3	C mks. off 3, leaving AC (4−3)=1.
3rd ,,	A ,, single	C scores 5+1=6.

Thus AC start the 2nd game with a single and 6 points to the good. The usual method would have left them with 4 tricks inscored.

Let us take one *extreme* example of the English game with and without honours, and the Chilian game:—

Counting Honours.

1st Game.—1st hand. AC win 4 by tricks, } AC win treble.
2nd ,, *AC* ,, 7 ,,
2nd Game.—1st ,, AC ,, 4 ,,
2nd ,, *BD* ,, 1 trick + 4 honours, BD win single.
3rd Game.—1st ,, AC ,, 4 by tricks.
2nd ,, *BD* ,, 1 trick + 4 honours, BD win single.

Thus BD win the rubber (1 point) with 2 tricks only gained against A-C's 19.

Not counting Honours.

Let BD in the 2nd hand of the 2nd and 3rd game win 5 tricks. They then win one point with 10 tricks against A-C's 19.

Chilian Method.—*AC* instead of BD win (8 points), 19−10−1 as they ought to do, though it is very unusual to lose the rubber and yet win points.

To mark a high score, it is not necessary to use many counters ..... =5, :... 6, .:.. =7, &c., &c., or any better way.

It is an objection to win the rubber and yet lose points, but this is not so objectionable as to win the rubber with 2 tricks against 19, or even with 10 against 19. Dicky.

Dear Five,—I enclose a letter from S. B., asking how "The Great Vienna Coup at Double Dummy," is to be solved. Will you give the problem next week? I thought two or three years ago I had solved the problem, but I cannot at the moment recall how it is to be solved, if, in the fourth round, *Y* discards a Heart. For, then, if *A* leads Heart, and *B* (dummy), after taking four tricks in Hearts, leads the long Heart, how is *A* to play? If he play small Diamond, *Y* throws a Diamond and makes his King of Spades in the last round; if *A* play Spade Queen, *Y* throws a spade and makes his third Diamond in the last round. Of course it is all plain sailing enough if *Y*, at fourth round, discards either a Diamond or a Spade. I take it this is S. B.'s difficulty, as he says he can see how twelve tricks can be taken but not the thirteenth. Editor.

PROBLEM IV.

Great Vienna Coup at Double Dummy, as given by Clay.

A leading is to make every trick.

A.
Clubs—A, K, Q, 3.
Hearts—2.
Spades—A, Q.
Diamonds—A, Q, 7, 6, 4, 3.

B.
Clubs—7, 4, 2.
Hearts—A, K, Q, Kn, 3.
Spades—Kn, 10, 3.
Diamonds—5, 2.

The Hands.

Y.
Clubs—8, 6, 5.
Hearts—10, 9, 7, 6, 5.
Spades—K, 6.
Diamonds—Kn, 10, 8.

Z.
Clubs—Kn, 10, 9.
Hearts—8, 4.
Spades—9, 8, 7, 5, 4, 2.
Diamonds—K, 9.

W. F. The term Fourchette is used by Cavendish for the two cards above and below a card already played. Thus, if ten of Clubs is led, and second player has nine and Knave of Clubs, he is said to hold the Fourchette to the ten.

D. O'C. Miley.—Under what limitations would you play what you style the "intermediate call?" For instance, your partner leads King, followed by Queen of a suit, in which you hold nine, three, and two, and you recommend throwing the nine and three instead of the three and two in signalling. But might not that leave him in doubt whether one of the adversaries was not signalling too? Thus, suppose the cards to fall thus, King, Ace, nine, seven, and when your partner again gets the lead that the next round in the suit falls thus Queen, four, three, eight. Your partner sees clearly that you have signalled. But the two has not fallen, and he does not hold it himself, nor, obviously, does fourth player. Would he not naturally infer that second player held the two and had commenced a signal (though he had already had a lead, and had not led trumps)? It happens at times that a double signal is given in this way. How is your partner to know it has not happened in such a case as I have suggested?

Lord Lytton as a Whist Player.—Lord Lytton was very fond of whist, and he and I both belonged to the well-known Portland Club, in which were to be found many of the celebrated players of the day. He never showed the slightest disposition of a gambler. He played the game well, and without excitement or temper, and apparently his whole attention was concentrated upon it; but it was curious to see that at every interval that occurred in the rubbers he would rush off to a writing-table, and with equally concentrated attention proceed with some literary work until called again to take his place at the whist-table. There was a member of the club, a very harmless, inoffensive man, of the name of Townend, for whom Lord Lytton entertained a mortal antipathy, and would never play whist whilst that gentleman was in the room. He firmly believed that he brought him bad luck. I was witness to what must be termed an odd coincidence. One afternoon, when Lord Lytton was playing, and had enjoyed an uninterrupted run of luck, it suddenly turned, upon which he exclaimed, "I am sure that Mr. Townend has come into the club." Some three minutes after, just time enough to ascend the stairs, in walked this unlucky personage. Lord Lytton, as soon as the rubber was over, left the table and did not renew the play.—From Sergeant Ballantine's "Experiences of a Barrister's Life."

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